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LECTURES
ON
COMPARATIVE ANATOMY
AND
ANIMAL PHYSIOLOGY,

DELIVERED BY

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LECTURE XI.

ON THE ORGANS OF SUPPORT OF THE CEPHALOPODOUS MOLLUSCA.

THE skeletons of the cephalopods, the highest of the invertebrated classes, present much greater variety of form, and characters which are more interesting, than we find in any of the other molluscous classes, chiefly on account of the circumstance that we here trace the gradual disappearance of the exterior unorganised shells of inferior animals, and the commencing development of the internal, organised, osseous skeleton of the vertebrated classes. There are no sudden transitions in the development of important parts and systems of organization, when we discover the various stages of their development, and the order followed by nature in their formation. As we find, in the class of fishes, remains of the external shells, in the form of calcareous scales, or plates, or solid spines, so we find in the cephalopods the first soft cartilaginous rudiments of the vertebral column, which lead us gradually to the still-imperfect condition of that central part of the osseous system which is met with in the *myxene*, the *lamprey*, and others of the lowest cartilaginous fishes. But it is very obvious that the organs of support serve the same purpose, whether they be deposited in the interior of the soft parts, or thrown out upon their surface, and the distinct nature of the organised and unorganised skeletons is shown here by the simultaneous existence of both in the cephalopods.

In the *testaceous*, or lowest forms of this class, the shells appear in the form of cones—sometimes straight, as in *belemnites* (*I** 4), *baculites* (*I** 2), *orthoceralites* (*G**), or curved, as in *hamites* and *scaphites*, or convoluted and orbicular, as in these recent shells of the *nautilus* (*F** 2) and *spirula* (*H** 1), or turbinated, like the shell of a gasteropod, as in these fossil *turrilites* (*I** 3), and in many of the minute foraminiferous genera, or with the chambers compressed into a straight calcareous dorsal lamina, as in the *sepia* (*F** 1). In many of the recent foraminiferous cephalopods, the shell is internal, polythalamous, thin, and horny, and in others the imperfect division of the chambers approaches to the delicate, horny, internal shells of the *pteropods* we were examining yesterday. Sometimes the chambers communicate by a prolonged calcareous siphon, sometimes by one or more simple foramina in the partitions. The shells of this class are sometimes placed on the outer surface of the mantle as in the nautilus, but most frequently they are covered by that part as in the foraminiferous genera, and in the larger naked decapods, *loligo*, *loligopsis*, *sepiola* (*K* 1). The shells of these swimming mollusca are always thin and light when compared with those of the creeping gasteropods. They are sometimes hard and calcareous, as in the nautilus and sepia, and often thin, diaphonous, elastic laminæ; but they never contain a trace of phosphate of lime, only carbonate. These hollow cones are generally more or less convoluted, as you observe in these recent specimens of the *argonaute*, the *spirula*, the *nautilus pompilius*. But a very different appearance is presented in the cone of the *nautilus* (*F** 2) from anything

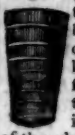
you meet with in the cones of the gasteropods. It forms internally a series of distinct chambers (*F** 2, *c*), thus composing what is called a multilocular or polythalamous shell. The body of the animal is contained in the last formed of these chambers (*F** 2, *a*), which is, therefore, much larger than any of those that preceded it. These chambers are formed, apparently, by a raising of the whole body of the animal from the floor of the last chamber, and remaining for a time at a certain distance from it, thus forming a new concave partition, the muscles of attachment creeping along the surface of the shell, as the muscles of attachment of gasteropoda and conchifera. The chambers are formed in succession in this way, as you see in the polished sections of this multilocular shell. The term "polythalamous" means the same in Greek as does "multilocular" in Latin —many-chambered.

These, then, are thrown out by the surface of the mantle, and, as you observe, beneath a thin deposit of epidermis. On removing this epidermis and the coloured opaque outer layer, you come to the beautiful shining nacreous layer, which may be observed in the specimens before us. These shells have the same texture and composition as those of gasteropoda. They consist of carbonate of lime, with animal matter, formed by successive deposits. The chambers communicate with each other by a canal or siphon (*F** 2, *b*), which passes through the whole series of partitions. In some the calcareous matter of the shell actually dips down through the whole of the chamber, reaches the commencement of the next funnel, and extends into it. Such is the case, you perceive, in this *spirula* (*H** 3 *c*, *a*), one of the existing cephalopods.

This shell, then, of the mantilus, is of the same compact and solid texture, and consists of the same calcareous elements, as we have seen entering into the composition of the exterior shells of the gasteropods, and all the other molluscous classes of animals. It is, therefore, that part of the skeleton of this cephalopod which is analogous to the shells of the gasteropods, and not to the internal articulated skeletons which we find characteristic of the vertebrated classes of animals. Thus, we observe, that its external opaque surface has the usual layer of colouring matter of the gasteropods, and a thin superficial layer of very thin epidermis, like a varnish; that its interior is a blueish-white, nacreous texture, and not the loose, fibrous, organised texture which we should expect to find were it at all related to, or the true analogue of, the internal articulated skeleton of the higher classes of animals. We observe that it still envelops the body, which, in fact, is entirely contained within the last-formed chamber of the shell; this large cavity, which is perforated in the middle of the base by a siphon—a small funnel-shaped opening that extends downwards into the next chamber; and so again each partition is made to penetrate the next chamber by a calcareous funnel or a small prolonged opening, until the whole divisions of the shell are gone through. This we found to be the ordinary structure of the polythalamous shells which are so closely connected with the peculiar organization of the cephalopods. On looking at these sections of the shells of the nautilus, you will be able to see the successive stages of the development of the shell, from the earliest periods of life. You will distinctly observe, that it is constituted of smaller and smaller chambers as you trace it backwards (*F** 2, *c*); that at the earliest period there was only a small monothalamous or unilocular cone which enveloped the posterior part of the body, like the shell of a gasteropod, to which, however, by the animal's rising a little forward, and advancing its muscular attachments, larger and larger cones or chambers were formed as the animal grew larger. So that it is like the ordinary shell of a gasteropod with the usual layers placed at a distance, and the interstices communicating. It sends down a blood-vessel, and a membranous tube through those perforations (*F** 2, *b*), but the use of which is still a matter of inquiry. Although many conjectures have been made as to its purposes, no one has had an opportunity of satisfying himself concerning the use of this perforation, which extends through the successive partitions and the chambers

of the whole shell. It had been conceived, and perhaps we know of nothing to contradict the supposition at present, that the membranous tube, which you see passes through this siphon of the shell, is part of an air bag; and that the animal might come to the surface to convey fresh air into the interior of this bag, by which means it might be enabled to swim or float easily at the surface, and there to remain until alarmed, when by expelling a quantity of the air it would sink and escape from danger. The vascular parietes of this membranous siphon and lining of the posterior chambers might even secrete some gaseous fluid, as is most frequently done by marine floating animals, and allow a portion of it to escape when necessary, as by the *ductus pneumaticus* of fishes. Perhaps many points of this kind may be settled by a more careful study of the minute testaceous cephalopods which abound on all the shores of Europe, or by observing the living habits of the recent species of nautilus which frequent the coasts of Britain, and which have been found alive on various parts around this and the adjacent islands. As this membranous canal of the siphon communicates with the cavity of the mantle and the exterior grooved funnel of the nautilus, there is a free communication between these solid compartments of this convoluted shell, and the surrounding element in which the animal swims, or with the atmosphere when it mounts to the surface of the sea to float.

The convolutions of this shell are all in the same vertical plane which would divide the animal into two symmetrical halves, and they are so expanded laterally, that the last-formed turn of the cone conceals all the preceding. In some of the nautili, however, the turns are exposed, and then they approach to the form of the ammonites (*I**, 1), the spirula (*H**), and similar orbicular cephalopods, where all the turns are distinct. The position of the siphon, which is here in the middle of each septum, varies in these convoluted shells, being sometimes on the outer or convex part of the shell as in the ammonite, and sometimes on the inner or concave side as in the spirula (*H** 3, *e*, *a*). Sometimes there are numerous round openings in the septa, giving passage to so many membranous siphons. From this structure of the nautilus, it is obvious that if its convolutions were uncoiled, it would form a straight, lengthened, tapering, polythalamous cone, with a large chamber at the broad open base, in which the animal is always placed and fixed by its two lateral muscles of attachment. Now this straight form of the cone is very common among the fossil polythalamous shells as in these *belemnites* (*I** 4), *baculites* (*I** 2), and *orthoceratites* (*G**), and it is easy to conceive



the last wide, large, upper chamber of these straight shells, occupied by a cephalopod of this kind, which carried them to and fro in the sea, or floated with them on the surface. We see indeed this straight conical form of the shell common among the existing light, floating, and swimming pteropods, as the *cresseis* and *cuvieria*. In the existence of these thin, hard, calcareous shells on the larger testaceous cephalopods, and mere horny, pellucid, delicate coverings on the minuter recent species of foraminiferous cephalopods which swarm on the shores of all latitudes, we see an analogy with the coverings allotted to the quick-moving and swimming crustacea. The entomostracous and other minute crustacea, which every where swarm through the fresh waters and the ocean, like insects through the atmosphere, have only a thin, transparent, elastic, horny shell, while the larger forms which seek their prey at the bottom, are provided with a very hard, though thin, calcareous shell. And the same remark, we have seen, applies to the gasteropods and pteropods, and all quick-moving invertebrated animals. To a certain extent, there is an analogous difference of character in the living organised skeletons of the vertebrated classes.

Among the cephalopods there are many convoluted shells where the turns of the cone are not in contact with each other, as we saw in the *vermetus* and *scalaria* among the gasteropods. This little *spirula* (*H** 1) is a shell of that kind belonging to an existing cephalopod—a cephalopod, however, concerning which we know still less than concerning the nautilus. The spirula is a polythalamous shell, the convolutions of which turn on the same plane, and are not in contact with each other; the chambers all communicate with each other by means of funnel-shaped continuations from each septum, which dip down into the funnel of the next septum (*H** 2, *a*; 3 *e*, *a*). In

*H**

the nautilus they extend but a short way (*F** 2, *b*), but in the spirula they extend

from the one septum down, passing the boundary of the next septum, and dipping into the funnel-shaped prolongation of that septum; so that upon making a section of the spirula, you see a continuous tube of a solid earthy matter, with only a slight interruption, which occurs at the part where they are inserted into each other (*H* 3, a*). This shell is supposed to project from the middle of the back of the spirula. I have examined the small mutilated portion of the mantle of the spirula preserved in the British Museum in spirits, and I find that the surface of the shell in that specimen has the same reticulate markings (*H* 1, b*) which we observe on the small fragment of the mantle which is preserved along with it in the vial. That appearance would render it probable that, like the shells of pteropods and foraminiferous cephalopods, it actually was embedded in the mantle which has given it the reticulate marking on the surface. Were we to stretch out this spirula into a straight line, we should have an appearance of the smooth, arched, and parallel septa (*H* 2; 3 b*), pretty much resembling the shell of the nautilus stretched out in the same way. It differs in this, that in the nautilus the siphon occupies nearly the middle of each septum, whereas, in the spirula, it occupies the interior margin. There is another shell like this, orbicular and convoluted on the same plane, the ammonite, which has the siphon in the opposite part to that of the spirula, on the exterior margin of the convolutions.

Pursuing the various forms of the shell, we come to those of the cephalopods, in which it is entirely concealed within the substance of the mantle. You observe it is so in the *sepia officinalis* (*F* 1*). It occupies a cavity in the middle of the back (*F* 1, a*)—a wide cavity in the fleshy substance of the mantle. The muscles which compose the mantle are not inserted into this lengthened, broad, laminated, friable, calcareous mass. It is a mass of the carbonate of lime, which is deposited, with condensed animal matter, in layers,—the first layers being those at the outer and lower part of the shell. The last-formed layers are those on the inner and upper part of the shell.

In addition to the carbonate of lime, which composes the greater part of this shell of the sepia, there is a considerable quantity of animal matter, which is seen by dissolving the carbonate of lime in diluted acids, which leave the animal matter untouched in the solution, consisting of an aggregate of thin membranous folds. There is a considerable portion of a dense elastic substance, like coagulated albumen, which forms a strong elastic layer on the outer surface, covering entirely, and passing beyond, the dorsal aspect of the shell. Were we to remove from this dorsal lamina of the sepia, or the bone of the cuttle, as it is commonly called, all the solid calcareous matter, you would only have this exterior elastic strong layer, resembling the epidermis of external enveloping shells; and there are some of the naked cephalopods which have, in fact, nothing but this horny elastic layer left, forming the last remnant of the external, unorganised skeletons of the gasteropods and other invertebrata. The form of this thin cartilaginous dorsal lamina varies in each species of the naked cephalopods. It is shaped like a feather in *loligo vulgaris*, more expanded above in *loligopsis guttata*, stiliform and short in *sepiola vulgaris*, and wanting or divided into two lateral portions in *octopus ventricosus*.

But the shape and the structure of this dorsal calcareous remnant of the shell of the gasteropods, found in the mantle of the sepia, are interesting in an anatomical point of view. You can see in the class of gasteropods, that there is a tendency to a deposition of the solid parts in the middle of the back. We see that in the snails, in the aplysia, the haliotis, the nucleo-branchia, and many other of the gasteropods, some of which appear to be naked, but have a solid calcareous or horny substance within the skin itself, not upon the outer surface. It is not at all remarkable, then, to find this calcareous shell confined in the skin of the back of the sepia. It is just the part where we would expect to find the last remains of the external unorganised skeletons, from examining the position of the shells in inferior molluscous classes. It consists of layers (*F* 1, a*) which are deposited as in the gasteropods, the one above the other, and the newest on the internal surface. These layers actually begin in the sepia by forming a small hollow shell, which receives into its interior another hollow shell, and which forms the second layer. This receives within it a third and a fourth hollow shell; and so they go on in the first stages of its growth. There is a considerable quantity of animal matter in these first-formed hollow shells, which project from the bottom of the dorsal lamina. After the shells have remained some time detached from the body of the animal, rolling at the bottom of the sea, this projecting convex part is found to be abraded, and you lose perhaps the most interesting part of the shell. You see by this, that in the embryo state it formed a hollow cone in beginning its shell, like the cone of the gasteropods; but as the animal grew up, the confined condition of those cones in the dorsal sac

of the mantle, caused them to assume a flattened form, which becomes more and more manifest as you ascend from the small projecting convex apex to the adult state of the shell. The successive new layers do not extend down to the lowest termination of the preceding laminæ, but always extend upwards beyond the superior margin of the last-formed layer; so that there must soon come a period when the middle part of this shell has the greatest diameter, and when it is thinner at the upper and the lower parts. If you make a transverse section of the shell, you will observe, on looking at the cut surface, that there are, in the middle, some thirty or forty of these layers; that these white calcareous layers are at a visible distance from each other; that they have between them some small white fibres placed closely, and everywhere passing directly from the one layer to the other; so that these innumerable crossing fibres between all the separate layers of the shell, produce that spongy and porous appearance which you know to exist in this shell of the sepia.

You have already seen, that in the external polythalamous shells the layers, which were at a greater distance from each other, and which had the means of expanding and continuing to form cones through life, being altogether on the exterior of the body,—these layers or successive cones were pushed further and further from the body of the animal, as in the nautilus. You have here in the nautilus and spirula communicating from the one chamber to the other, only one tube or siphon, but in place of that one tube in this other specimen,—the shell of the sepia,—we find that these small white fibres, which cross from the one septum or layer of this flat shell to the other, are so many minute calcareous tubes. There are thousands of these minute tubes or siphons passing through all the partitions of the shell of the sepia, and there are often many communicating perforations in the septa of foraminiferous species.

It is interesting to consider the structure of this shell with relation to the existing and the instinct forms of the cephalopods. You will observe that it occupies the middle of the back, and in such a manner, that if, like the little spirula, it had pushed its successive cones from a cavity in the back, the first-formed, the central cone of the disk,—would have been that projecting at the outer and lower part of the shell of the sepia. The convex hollow layers, like cones, continuing thus to extend outwards, and with an oblique direction, turning on the same vertical plane, would have formed a convoluted suborbicular shell, like that of the ammonite or the nautilus. Where, then, in this so convoluted shell of the sepia, would have been the convex outer border of the shell, or the upper lip, with relation to the rest of the body of the sepia?

The sepia would have looked forwards over the spire from the last-formed chamber, which we may suppose to have enveloped the whole trunk of the animal. It would have looked from the chamber forwards, over the columellar lip, keeping the exterior convex part of the chamber or the upper lip extending across its back, as in all other known orbicular shells of pteropodous and gasteropodous mollusca. The shell of the sepia has its layers disposed as you observe here,—the smaller and first-formed at the outer and lower part, and the successive layers deposited within, extending upwards; so that if you could uncoil those revolutions of the shell of the nautilus, and then press the apex towards the base, you would bring all these septa nearly into contact with each other, and you would produce a flattened polythalamous shell like that contained in the dorsal sac of the sepia, where the shell you perceive is quite unconnected with the muscular parts around it. So that there appears to be a very close analogy in the nature, and probably also in the position, of these two shells, which are the most remote from each other in their outward form.

The exact position of the animal in the recent nautili, spirulæ, and other polythalamous spiral shells, although important in the interpretation of the fossil forms, has not yet been satisfactorily observed by naturalists. In our present uncertainty, therefore, regarding the position of the living cephalopods in these convoluted shells, we can only be guided by analogy; and these are the analogies of structure which I have laid before you. You can also derive analogies from what we have already observed in gasteropods and pteropods. You have a specimen before you in the class of gasteropods, of a shell which revolves upon the same plane, in the common *planorbis*, which abounds in every stagnant pool around London. When this animal extends itself from the shell, you will observe, that the free margin of the aperture, or the upper lip, rests over the neck of the animal. In extending outwards its foot to creep on the solid surface, it extends itself in such a manner that the head with its tentacula extends from under the outer or superior convex margin of the shell, and that its abdominal surface is towards the inferior or columellar margin. From an examination of this circumstance, which is com-

mon to all other testaceous gasteropods, and from seeing that the limacina and other pteropods have the same relation to their shells, we have the structural evidence confirmed by all known analogies of similar forms, and we can thus form a conception of the manner in which the living animals were disposed in all these remarkable forms of the shells of the cephalopods with which we are familiar in a fossil state.

The ammonite (*I** 1) is a well-known orbicular polythalamous shell, like that of the nautilus, the spirula, and many other cephalopodous shells I have shown you, therefore we can trace this polythalamous character from the flat internal dorsal shell of the sepia down to those fossil, thin, convoluted shells of the ammonites. In the ammonite, however, you will observe, that in place of the partitions passing in a regular arched manner so as to bound the chambers (*I** 1, *b*) by a smooth concave surface, the margins of the partitions pass in the most irregular zigzag manner (*I** 1, *a*). That is marked by the beautiful zigzag lines which you observe upon the exterior surface of all these casts of ammonites, which are so abundant in the older rocks, where the calcareous layers of the thin original shell have entirely disappeared, and where you have only the exact cast, as is usually seen in these fossil cephalopods, of its internal cavity left in a solid rock. Were all the connecting calcareous matter removed from the septa of this ammonite, as the external shell is almost entirely gone, these chambers would be quite detached from each other, and only held in contact loosely by these serrated and dove-tailed kinds of margins of the chambers; and some upon looking at them in this loose and detached condition, have thought them the vertebral columns of serpents coiled up, and all these separate casts of the chambers as so many detached vertebræ, which they somewhat resemble. These isolated ammonite chambers (*I** 1, *b*) have all a small portion of the general curvature, their inner margin is shorter than the outer, and they have all that zigzag outline which we see in the chambers of the ammonite, but not in vertebræ. The siphon passes along the outer side of the margin, which is another means by which you can obviously recognise these detached chambers of the ammonite. It is probable from analogy, and from the magnitude of the last chamber, that these enormous ammonites and the other extinct polythalamous shells which resemble them were external, like the nautilus, and not internal shells, as you will find stated by most naturalists,—Baron Cuvier, Meckel, and others. They are light thin shells, which could enable the animal to come to the surface of the sea, like those of the existing testaceous cephalopods, and they required such dense light shells, that they might rise in the water to expand their tentacular membranes at the surface of the ocean, for the purpose of getting their food, or air for floating, or for better respiration.

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These ammonites, which are entirely extinct animals, are found in our own latitudes of a magnitude equal to a cart wheel. They far exceed in magnitude, though occurring in our own cold latitudes, anything known of the testaceous cephalopods on the face of the earth at present in the hottest regions of the ocean. You are not to conceive, that in those enormous remains of this animal, which you observe in museums, that you are looking actually upon the testaceous covering of the cephalopod that inhabited them. The original testaceous thin covering from such a specimen as this large ammonite before you has been entirely removed, and you have, as usual in these delicate shells, a cast of its interior only preserved (*I** 1), which is here a solid mass of siliceous limestone.

Were you to judge from zoological arguments touching the temperature and the other circumstances connected with, or favourable to, the high development of animal life upon this planet; taking only the cephalopods into consideration, you would say, that as the known testaceous cephalopods acquire a great development only in tropical seas, and that as we know of none of the innumerable existing species spread over all coasts, which have a greater size than the little spirula, inhabiting latitudes at all approaching to those of Europe, these enormous remains of testaceous cephalopods are indicative of the conditions now met with only in tropical seas; that from the vast proportion of the development of testaceous cephalopods in the ancient strata of our own latitudes, they would indicate that the then-existing seas had been of a tropical character, and that at the period when these enormous ammonites and other testaceous cephalopods

were thrown down the seas of European latitudes, had a temperature even exceeding that of the hottest regions of the present ocean. Now although these ammonites are the most abundant and the best known of fossil animals, although more than four hundred species are described by naturalists, and more than a hundred of these are met with in our own strata, although they occur in such abundance in a fossil state that whole strata of rock are composed of their shells, not one is now known to exist on the face of the earth. They retain their nacreous lustre in the oldest limestone rocks, and the beautiful lustre of the Carynthian marble is produced by the nacre of these shells. They present a structure which can be traced through a great variety of other forms of cephalopodous shells. You observe the *scaphite* has its two extremities bent up like the ends of a canoe, with the last enlarged turn detached from the rest, while the *hamite* has its extremities suddenly curved like a shepherd's staff or a hook, but has the same lobed chambers and external marginal siphon which you see in the ammonite. The *baculite* (*I** 2) presents the same internal structure, but its form is long, straight, and conical, and with a large upper chamber for containing the body of the cephalopod. In this fossil nautilus, or nautilite, as the fossil species are called, which I took from a chalk cliff at Rouen, in France, you perceive the same smooth concave partitions with the siphon in their middle, and still preserving their nacreous lustre, as in the recent species. The structure of the orthoceratite (*G**) is like that of the nautilite, but has its chambers disposed generally in a straight line, instead of forming a spire. Here are numerous straight, lengthened, conical shells, slightly compressed, and tapering suddenly at their closed extremity, which have very little of the external appearance of the polythalamous shells of cephalopods. They have more external resemblance to spines of the echini. These are *belemnites* (*I** 4), where the first-formed cone is at the narrow pointed part of the shell. The cones have been placed within each other, so that the last wide, cleft, upper chamber, contained the cephalopod, which in swimming could probably draw up its delicate shell to the surface of the water, as we see in the existing testaceous cephalopods. Here is another interesting form of these external skeletons, which I picked out from the chalk cliffs of Rouen - cliffs which abound with a great variety of the most interesting forms of testaceous cephalopods. In this specimen of the *turrilite* (*I** 3), you will observe that the convolutions of the shell have not turned upon the same plane as in the ammonite, but that they have constantly changed the plane of revolution at every instant in the same manner, and probably from the same unsymmetrical development of the two sides of the body in the animal it contained, as you see in the turbinated shell of a gasteropod.

Now, upon the first view of the exterior of this turrilite, you would suppose it to be a unilocular, spiral, turbinated shell of a gasteropod, but it is a polythalamous shell, and has an internal structure most closely allied to that of the ammonite, the circumstance in which it differs being solely, that it has this turreted form produced by the turns constantly changing the plane upon which they revolved. In seeking transitions from one form of skeleton to another, and the anatomist knows the importance of this in tracing developments in anatomy, whether human or comparative, such intermediate shells as this turrilite are very interesting. This animal has the shell with the outer form of that of a gasteropod, but it has the internal structure peculiar to the shells of the cephalopods. The internal chambers (*I** 3, *a*) here you see are separated by numerous lobed partitions, having at their margins precisely the same irregular outline as you observe in the chambers of an ammonite. These extinct testaceous skeletons of the cephalopods however are infinitely more interesting in a geological point of view than as relates to the existing forms. There are, in fact, a hundred forms fossilised to one which we have in existence. In ancient conditions of this plant, these larger testaceous cephalopods, which are now almost extinct from the face of the earth, have abounded so much, that their remains form whole strata of mountain-rocks; and these limestones, composed almost entirely of the shells of the ammonites, are most remarkable and beautiful. The shells that compose them retain, you observe, their nacreous lustre, which, with the transparency of the consolidating material by which they are kept together, produce the most brilliant displays of lustre, when polished sections are made of them. They are found particularly to abound in Syria, in Palestine, and in all that part of Asia. Some of these polythalamous shells, with smooth concave partitions, turn round their apex without covering the spire, as you see in the convoluted orbicular *rotalite*, flat, like a planorbis; some are convex and round, like a lentil, as the *lenticulite*, and others are round and flattened like a piece of money, as the *nummulite* (*I** 5), which has its small narrow convolutions divided internally into innumerable very minute chambers. This discord form of the nummulite is very common among

the smaller fossil testaceous cephalopods, and is so abundant as to constitute a great part of the so-named nummulite-limestone, of which the pyramids of Egypt are built, and on which these ancient monuments of human skill rest their foundations. From seeing these rocks composed of those small round bodies, it was once supposed that they were actually lentils; that they were the food of the workmen who had constructed the pyramids, and which were fossilised and left adhering to the rocks. We know now that they are the nummulites, with myriads of other minute shells of animals belonging to the class we are now considering.

The examples which I have presented to you will give some idea of the internal structure, and of the various forms of the polythalamous shells of the cephalopods. But this is the class of invertebrated animals that comes next to the class of fishes, and, consequently, that class in which we should look for some trace of the first development of those internal soft cartilaginous parts which we find composing the internal skeleton of the lowest species of the class of fishes. As the internal osseous organised skeleton is not suddenly brought to perfection at its first appearance in the animal kingdom, but begins by the development of its most rudimentary and essential parts, so we find that the external unorganised form of the organs of support is lost, not at once, but by successive degradations. We have seen that the external enveloping shell of the inferior testaceous cephalopods is gradually diminished, and is withdrawn into the back part of the body, and concealed loosely in a dorsal sac. Its calcareous consolidating part is next withdrawn, and only its horny elastic portion is left in the naked or most elevated forms of cephalopods.

We find extending down the middle of the back of many of the naked cephalopods, as I mentioned to you in the *loligo*, the *sepiola* (*K** 1, *a*; 2; 3), and the *loligopsis*, a thin cartilaginous lamina, flexible and pellucid, which is lodged in a distinct sac. You have already seen the analogue of that substance in the exterior horny covering of animal matter, which is here as a layer on the outside of the shell of the sepia. This is to be looked upon as a remaining trace of the shells of the cephalopods which we have seen leaving since we left the nautilus, and not as a rudiment of the internal vertebrated skeleton as Spix supposed. There are some of the naked cephalopods fortunately still left to us in existence, in which you cannot trace even this last soft remnant of the solid skeletons of the invertebrated classes; that is the case with this *octopus*. Now by watching the earliest development of the internal articulated skeleton in fishes, and in the embryo of the human body, you are aware that the first-formed parts are those which are to enclose and protect the spinal marrow. You know very well that when that primitive streak is produced in the germinal membrane, which we afterwards find to be the rudiments of the nervous system, there appear along its sides little opaque points, which are the first elementary parts of those vertebræ which are afterwards to compose a tube around the great centre of the nervous system. There are four principal parts which we observe in each of the separate vertebræ composing that tube; there are a *body*, two *laminæ*, and a *spinous process*, and in the middle between these we have a *foramen* or canal. You will observe, that in the lowest fishes where this internal skeleton is for the most part soft and cartilaginous, the *laminæ* are more consolidated, more developed than even the bodies themselves of the vertebræ. Look at this skeleton of the sturgeon. The *bodies* of the vertebræ form a soft, cartilaginous, transparent, flexible mass, where you can scarcely see a trace of subdivision; but look to the *laminæ* that are on the outer surface to shield the spinal marrow upon that surface and on the sides. You see how they are consolidated. Compare that also with what you know to take place in the earliest development of the human vertebræ. Would you not infer from these circumstances, not only that the vertebræ are the first-formed parts of the skeleton, but that the laminæ are their most important elements, being those first formed, and that therefore they constitute that portion of the skeleton which you would naturally look for, beginning to manifest itself first in those highest of the invertebrated animals.

On the back part of the neck of the *loligo* and other naked cephalopods, you observe two cartilaginous laminæ, to which the muscles are attached, into which they are inserted. These approach close to the cephalic cartilaginous plate which envelops the great supra and infra-œsophageal ganglia and through which the œsophagus passes. These are the external vertebral laminæ which are developed in the lowest cartilaginous fishes to a greater degree, but are not yet subdivided. They are very different from the dorsal, loose, cartilaginous laminæ which have been long known in the cephalopods, as that of the *loligo vulgaris* (*K** 3), that of the *loligopsis guttata* (*K** 2), and that of the *sepiola vulgaris* (*K** 1, *a*). That cephalic, cartilaginous, curved lamina which embraces the

great supra and infra-œsophageal ganglia which have been compared to the brain, is the analogue of the occipital bone or first cranial vertebra. It has a curved and expanded form, and is the portion of the cranial vertebræ which is next to the laminæ of what you might say were cervical or dorsal vertebræ. These parts are all of a soft and flexible nature, translucent, and of the consistence of cartilage. In no cephalopods do they ever acquire the solidity of bone, or even the solidity which they possess in the higher cartilaginous fishes. In addition to these ordinary parts, however, we can observe in many of the cephalopods cartilaginous portions, developing in different parts of the body.

You have here magnified figures of the *sepiola* (*K** 1), an animal closely allied to the sepia, having eight sessile arms (*K** 1, *d*) around the head, with two long tentacula (*K** 1, *e*, *e*), with a rounded caudal termination, and expansions on the sides of the body like fins for moving it to and fro in the sea. These lateral fins (*K** 1, *e*) are supported by curved cartilaginous laminæ (*K** 1, *b*, *b*), loosely attached to the back, and connected only by layers of muscular fibres which serve to move these rudimentary extremities.

*K**

We observe that in almost all the cephalopods that are naked, the portion called the mantle, that loose muscular fold which envelops the great viscera of the abdomen, is connected with the animal by two firm cartilaginous strong substances, two rudimentary clavicles which unite to the elements of a sternum. From the exterior of the mantle are seen the two cartilaginous, thin, flexible, transparent laminæ, which from shooting down from the region of the vertebral column may be looked upon as the rudimentary bones of a pair of extremities; and in my description of the anatomy of the animal represented in this diagram, the *sepiola vulgaris* (*K** 1 *b*), I have ventured to compare these cartilaginous portions to scapulæ; these cartilaginous curved plates moveable on the outer surface of the back of the sepiola vulgaris. That structure you see even to a greater extent developed, although less moveable, along the sides of the mantle in the common *sepia*. In this animal, the common *sepia officinalis* (*K** 1), there is a muscular fold extending along the whole side of the body (*K** 1, *b*), by the motions of which the animal is enabled to swim in every direction backwards or forwards. That muscular fold is supported internally by a cartilaginous lamina, which is perfectly the analogue of that which is here represented on the little sepiola vulgaris, the smallest of the naked cephalopods inhabiting the present seas.

From the position of these cartilaginous portions of various parts of the body that I have mentioned in the naked cephalopods, we may regard them as the first rudiments of the cranial vertebræ, of the rest of the vertebral column, although not yet divided into distinct vertebræ, and the rudiments also of the extremities. It might at first appear to be extending analogies beyond what there is just and strong argument to support, to compare those extremities or fins of the cephalopods with the anterior or posterior pairs of higher animals, but to consider this matter more curiously we might even trace some kind of analogy. Which parts of the body in the highest animals are first and most developed? The upper parts nearest the greatest rush of the arterialised blood from the placenta; whereas the lowest parts of the body are small and rudimentary, compared with these upper portions, which receive the greatest afflux of arterialised blood passing through the foramen ovale to the left side of the heart, from that through the arch of the aorta, and through the ascending branches that come from that arch; whereas the impure venous blood returned by the superior *vena cava*, and sent through the right ventricle and *ductus arteriosus*, enters below the arch of the aorta, and is conveyed to the lower parts of the body and to the placenta. These lower parts of the trunk, therefore, and the posterior extremities, being less nourished, are proportionally small. You see that disproportion in the posterior parts even of the kangaroo at an early period, although they afterwards acquire so great a size. Which of the pairs of extremities are most developed and most constant in fishes? The pectoral fins or arms. Which extremities are alone present in the *sirens* and most developed in the *proteus*? The arms. Which are largest in the *turtles* and aquatic *chelonia*? The arms for swimming. Which in the birds? The wings for progressive motion through the air. Which are alone developed in the dolphin, the grampus, the

whales, and other gigantic cetacea? The arms. The slow-worm (*anguis fragilis*) among the serpents has no arms without, but what rudiments of them do you find within? The scapular arch for the support of arms to be developed among the *sauria*. So that analogy appears to confirm what the structure only suggests, that those members developed from the sides of the trunk in the cephalopods, for progressive motion through the sea, are the first rudiments of the anterior rather than of the posterior extremities of higher animals, and that they shift their position forwards and backwards on the trunk of cephalopods, like the legs of fishes, which are not yet fixed to the vertebral column.

The jaws (*K* 5, a, b*) are very perfect in the cephalopods, and have the form of horny mandibles (*K* 5*) so common in the lower vertebrata, as in the chelonian reptiles and birds. They are curved, short, strong, of a dense texture, with sharp cutting edges in the naked species, and are destructive instruments among the young crustaceous and testaceous animals on which the cephalopods chiefly feed at the bottom of the deep. In the nautilus their edges are more broad, tuberculated, and consolidated with calcareous matter. In the vestibule of the ear, these animals have a conical calcareous body of a soft chalky consistence, as we commonly find in the lowest classes of vertebrata. And the surface of their tongue is covered with regularly-disposed, recurved, sharp, horny spines, which are common on that part, both in the invertebrata and in the vertebrated classes. The cirrhi, or arms (*K* 1, d*) developed from the oral disk, which forms the circular lip of the cyclostome fishes, are provided with circular cups or suckers, (*K* 4*), as we see on the tentacula of some annelides. These suckers (*K* 4, a, b, c*) of the cephalopods have their circumference supported by a cartilaginous ring, which presents numerous minute very sharp teeth (*K* 4, a, c*), moveable, and directed towards the centre of the sucker so as to aid that organ in attaching itself to the surface of soft animals. Sometimes, as in *onychia*, these cartilaginous disks are drawn out to the conical form of curved claws, as we see also on the oral disk of lampreys. These are principal forms of the skeleton and of the hard parts which present themselves in the class of cephalopods, the last and highest of the invertebrated division of the animal kingdom.

On Monday I shall proceed to the skeleton of fishes.

HOTEL DIEU, PARIS.

CLINICAL LECTURES ON SURGERY,

DELIVERED BY

BARON DUPUYTREN,

During the present Session, 1833.

(Revised (before translation) by the Baron himself in the fasciculi of his "Leçons Orales de Clinique Chirurgicale," published periodically by G. Bailliere, Paris).

LIGATURE OF THE EXTERNAL ILIAC ARTERY.

The ligature of the external iliac artery apparently presents greater difficulties than that of the subclavian. It is not, in fact, without some feeling of fear, that we reflect how the vessel must be sought, as it were, in the abdomen, to be tied; however, in practice, this operation is generally less difficult, and not more dangerous, than the other. The parietes of the abdomen are divided in the high operation for stone; they are divided, in almost every operation, for hernia; why not then open them without fear, in order to cure a disease, certainly as dangerous as any of the preceding ones? The place where the incision should be made, and the disposition of the peritoneum, enable us to expose the artery, and avoid injuring the serous investment of the bowels.

Several methods may be employed; the first consists in opening the parietes of the abdomen parallel to the direction of the artery, commencing at the point where it passes under the crural arch, and ascending along the external edge of the rectus, toward the umbilicus. In the second method, which is that of Abernethy, the abdominal parietes are divided parallel to the crural arch, and about half an inch above it. The process of Sir A. Cooper consists in making a curved incision above the arch, commencing from the anterior superior spine of the ileum, and terminating near the external ring. Boyer has proposed to make an incision of two inches, just above the arch, one extremity of which shall be at the same distance from the spine of the ileum as the other is from the spine of the pubes; the tendon of the great oblique muscle being opened parallel to Poupart's ligament, the operator pushes aside the chord, and soon comes down on the iliac artery; but this incision, made exactly over the direction of the artery, does not permit us to tie the vessel more than an inch above the arch; by turning the lower angle of

the incision toward the iliac spine, we should gain more room, but then we should fall into the method of Cooper, with all its inconveniences. The incision, practised after the first method, gives an opening parallel to the vessel, but this opening is limited to the separation allowed by the lips of the wound, which renders it very difficult to find the artery, or to perform the manœuvres necessary for placing the ligature round it. Besides, in making this incision, we run the risk of opening the peritoneum, which, in proportion as it is distant from the arch, adheres more closely to the abdominal parietes. The incision made parallel to the crural arch being perpendicular to the iliac artery, renders all parts of the operation more easy, in addition to which it falls precisely on the line where the peritoneum abandons the parietes of the abdomen to be reflected on the pelvis, and in the midst of loose fatty cellular tissue always found in this situation; it also gives greater facility for detaching the peritoneum, and exposes the patient to much less danger of having this membrane opened; however, if continued too far toward the ring, it may divide the epigastric artery.

The ligature of the artery, by one or other of these methods, may be made more or less high. In tying the vessel high, we get at a distance from the tumour, but we increase the danger of wounding the peritoneum; in taking up the artery low down, we avoid the danger, but then ligature is placed so close to the aneurysmal sac, and to the origin of the epigastric artery, that the former may be attacked by secondary inflammation, and open into the wound, while the lower tube being traversed by the blood of the epigastric artery, may re-establish the circulation and pulsations of the aneurysm. In each of these different processes, the artery is isolated from the vein and nervous plexus, which lie on its inner side, with the finger rather than with any instrument; the laxity of the cellular tissue fortunately renders this operation easy, which might otherwise be dangerous if practised with a cutting instrument. It is much more easy to find the termination of the external iliac artery in the woman than in the man. In the former it is situate more superficially, on account of the peculiar form of the female pelvis; but whatever be the sex, the operation is more easy in thin than in fat individuals. The following case will give us an opportunity of applying some of these principles.

Case.—*Aneurysm of the Femoral Artery.—Trial of Pressure and Cold on the External Iliac.*

Berger, an old soldier, now a stone-cutter, 45 years of age, in the month of June 1815, made a sudden effort to lift up a heavy plank, one extremity of which rested on his left groin; he immediately felt in this part considerable pain, but as it was of short duration, he continued his occupation for several days; however, in about two months Berger perceived a small tumour in the left groin, about two inches below the crural arch: it was perfectly indolent, and therefore attracted no attention from him. By degrees the tumour enlarged, up to June 1816, when the patient, having made some fresh exertion, it suddenly increased to the size of a hen's egg. About three weeks before his entry into the hospital, the poor man fell on the edge of a copper vessel, and by some fatality the weight of his body was brought to bear exactly on the tumour, which enlarged so rapidly from this period, that he judged it prudent to enter the hospital in August 1816.

The tumour, placed as I have said, in the left groin, over the femoral artery, had at this time the form and size of a large pear, with the base turned upward, and the point downward and inward. It commenced a little above the arch, and extended down for four inches: being two inches and a half in breadth, it did not present any pulsation in mass, but alternate dilatations and contractions, quite isochronous with the movements of the heart. When the external iliac was compressed on the branch of the pubis, all kind of movement in the tumour was suspended, and at the same time it became sensibly diminished. If the femoral artery was compressed in the middle of the thigh, the pulsations, on the contrary, became more strong and violent. When the tumour itself was compressed, it disappeared altogether, and we could feel that its parietes were very unequal, and of the consistence of cartilage. On the least suspension of the pressure, the tumour recovered its ordinary volume and tension in two or three beats of the heart; besides, it was perfectly indolent, and the skin was not in the least discoloured. It was impossible to doubt that the tumour was an aneurysm of the femoral artery, and as it had latterly made such rapid progress, it was equally certain that it was necessary to do something speedily.

Two methods of treatment were applicable to this case,—compression and the ligature. If pressure were unsuccessful,

it would at least favour the success of the ligature; we therefore decided on applying pressure to the tumour, aided by cold. Being convinced, by experience, how easy it is to compress the external iliac artery on the horizontal branch of the pubes, I caused an instrument to be made by one of our most intelligent cutlers of such a nature, that one end pressed on the sacrum, the other upon the iliac artery. The machine being applied about half an inch above the arch, the circulation was perfectly suspended in the arteries below this point, as well as in the tumour, upon which we placed pounded ice in a bladder. All the surrounding parts were guarded from the action of the ice by folded cloths; but we soon found the imperfection of this instrument. The pulsations of the tumour, which were suspended when the instrument was first applied, soon reappeared whenever the patient coughed or spoke, or made the slightest motion; and even when the pressure was exact, it excited such uneasiness, that the patient was unable to support it above fifteen minutes at a time if the ice were at the same time used, while without the ice it could be borne for half an hour. These pains, which were evidently due to the pressure of the crural nerves, ceased in about five or six minutes after the application of the machine. It was frequently altered, but as it always had the fault of not following the motions of the pelvis, and of gliding off the horizontal branch of the pubes, when the patient moved in bed, it could not exercise an efficacious pressure.

This compression was continued without intermission to the 18th of September, but it became so painful that we were compelled to abandon it; however, the tumour was sensibly diminished. The patient was permitted to get out of bed for a few days, and experienced on getting up some uneasiness in the left knee-joint, which disappeared in the course of forty-eight hours.

In spite of these difficulties I did not give up the hope of conquering the disease by means of compression. A new machine somewhat analogous to Cowper's truss-bandage was made; it consisted of an elastic steel plate, forming about five-sixths of a circle; one extremity was fixed on the right haunch; the other, which was more straight, and twisted a little upwards and to the left, was furnished at about six inches from the extremity with a pad corresponding exactly with the point where the external iliac artery passes over the horizontal branch of the pubis. The whole action of this instrument depended on its elasticity.

It was applied on the 20th of September, and we soon perceived this advantage in its use, that it accommodated itself to all the movements of the body, by which means the compression was rendered sufficiently fixed and exact to have attained with certainty the proposed object, if the patient had been gifted with a little more courage and endurance. The ice was applied again, and the action of the truss was regulated by means of a strap passed under the thighs, but it could not be borne by the patient longer than the former one, and ten days after its application Berger demanded the operation with great earnestness. In vain I attempted to impress upon his mind the advantages of a means by which he might hope a cure without an operation; he constantly refused to employ the pressure, and, at length, wearied by his entreaties, I appointed a day for the operation. In the intermediate time we endeavoured to calm the state of nervous excitement into which the compression had thrown him, and we made such dissections and experiments on the dead body as were calculated to render the operation on the living more successful.

At this time the tumour was diminished to one-third of its size, and the force of the pulsations remarkably decreased. On the 9th and 10th of October the patient experienced pain in the inner side of the tumour, and extending along the thigh to its middle; they ceased on the 13th, but there still remained some pain in the back part of the thigh, corresponding with the direction of the sciatic nerve.

As the patient was habitually costive he was ordered on the 14th and 15th emollient clysters, which brought away only two small stools of dark and hard matter; as the operation was to be performed on the following day, I prescribed an ounce of the *sirup of diacordium*, which procured a tranquil sleep for six hours.

The instruments used during the operation were various straight and convex bistouries, a straight bistoury boutonnee, a grooved sound, two needle-shaped stylets threaded with waxed threads, forceps, scissars, several small cylinders of linen, and sponges.

The patient lying in an horizontal position, an assistant was placed by his side, in order to suspend the course of the blood in the lower extremities, by pressure on the termination of the ventral aorta, if such should be necessary.

I now commenced an incision, beginning about an inch below the superior anterior spine of the ileum, which ran parallel to the crural arch, and extended to the inguinal ring; the spine, tendon,

and muscles, were successively divided with great caution, and I soon came down on the cellular tissue, which was yellow, slightly injected, remarkably dense, and containing a great number of lymphatic glands, with which it adhered strongly to the external iliac artery; this occasioned some difficulty in exposing the vessel, for we were compelled, not only to remove this cellular tissue, layer by layer, but also in order to favour this delicate dissection, we were obliged to divide the crural arch by several small perpendicular incisions; when the artery was cleared externally of the cellular tissue, and separated internally from the iliac vein by means of the index finger with great caution, it was elevated on both index fingers, while an assistant passed underneath it a hollow sound. When the artery was compressed on this sound, all pulsation was suspended in the tumour. A ligature was now placed, almost an inch above the diseased part, by passing a stylet through the canula; a ligature of reserve was placed half an inch higher up, by the same means; the artery was raised by drawing gently the two ends of the ligature, while the index finger of the left hand was pressed on the knot. This manœuvre again suspended the circulation; the vessel was tied at once, and the pulsations of the tumour immediately stopped.

A remarkable circumstance occurred during the operation; while the patient contracted forcibly the muscles of the abdomen, the edges of the wound were almost brought into contact, and the peritoneum, pushed forward by the intestines, presented itself under the blade of the bistoury; we were thus compelled to direct an assistant to push back the intestine and peritoneum with his fingers, while he kept the edges of the wound open.

The inferior ligature was placed at the lower angle of the incision; that of reserve in the upper angle. The wound was covered with linen spread over with cerate, and above this were placed several folds of fine lint. Triangular compresses and bandage completed the dressing.

Thus terminated, without much difficulty, the ligature of the external iliac artery, and if we were to judge from the effects which followed the ligature of the subclavian, we had cause to hope that the patient would soon be perfectly cured. But, instead of this, a long series of accidents set in, which I propose to explain to you at length; first, because they are essentially connected with the case, and, secondly, because they show how necessary it is to follow up an operation by judicious treatment. The details shall be comprised in another division.

A LECTURE

ON A

PECULIAR KIND OF INJURY OF THE SHOULDER-JOINT,

Delivered at the Westminster Hospital,

BY MR. GUTHRIE.

Gentlemen,—The observations I am about to make, constitute an essay which I reserved for a time, in the hope it might find a place in the first volume of the Transactions of the Royal College of Surgeons of London. But as my expectations in this respect have hitherto proved vain, and as there is at present in the hospital an example of the kind of injury I wish to illustrate, and which affords you an opportunity of judging of my opinions, I have thought it right to deviate from my original resolution. It is possible, that when attention is particularly and generally drawn to the subject, an opportunity may be found of ascertaining by dissection the exact nature of an accident which has caused me much anxiety, and which may have been a source of inconvenience even to those who are best qualified to form a judgment on these subjects. As I wish on this point to be fully understood and fairly represented, I will, if it be agreeable to the gentlemen who take notes for the medical journals, and they think it worth their while to accept the offer, give them an opportunity of copying my own remarks on the subject—in fact, the very paper I now hold in my hand. I have no interest in any journal, save that which arises from civility; and there is an old saying, that "civility begets civility."

Louisa Chapman, eleven years of age, fell on her shoulder on the 8th of October last. This child's shoulder is, as Mr. White aptly calls it, out of drawing. There is a considerable prominence on the inside of the anterior part, beneath the coracoid and acromion processes, and in the situation of the small tuberosity of the humerus. This projection is so manifest, as to be seen at a distance; and when examined, it might be mistaken for the head of the humerus displaced into this situation, and constituting a partial dislocation, if it were not for two circumstances. 1st, That it is not the whole head of the humerus which is felt projecting internally; neither is it round, but, on the contrary, it is distinctly a rough and irregular protuberance of bone. 2ndly, That a portion of the humerus may be felt in the glenoid cavity.

If the thumb or fore-finger of the left-hand be placed on the internal protu-

berance of bone, whilst the elbow is rotated by the right, the protuberant portion of the humerus is found to obey the motions given to the elbow, in the clearest manner possible. If the fore-finger be carried outwards, and placed on that part of the joint in which the external half of the head of the humerus ought to be placed in a normal state, it will be found there, and moving most distinctly under the finger when the elbow is rotated. The arm can be elevated, and the hand placed on the top of the head. There is no peculiar or decisive sensation communicated to the fingers when pressed into the axilla, and the elbow can be brought close to the side with ease, although it tends a little backwards. If the point of the fore-finger be placed immediately below the middle of the acromion process, it sinks into a hollow, between the protuberant point of bone and the articulating part of the humerus which moves in the glenoid cavity, and if the two shoulders be compared, the greater width of the injured one is apparent. What is the injury? Dissection has not yet explained it, but I believe that it is *a longitudinal split of the humerus*. The accident always happens in consequence of a fall on the point of the shoulder, in which the head of the humerus, covered by its investing parts, first meets the ground, and receives the shock. It occurs, then, from direct violence committed on the part. In some cases there is an indistinct crepitus; and in one case, which I shall presently quote, the whole head of the bone was dislocated as well as split. I believe the split separates the small tuberosity, with more or less of the head, and extends in the direction of the occipital groove; and I suspect that the tendon of the pectoralis major in front, and those of the latissimus dorsi and teres major behind, prevent displacement by acting in the manner of a hinge. The crepitus is not always distinct, especially in young persons, on account of the cancellated structure of the bone being soft, and its interstices being filled with medullary matter. In this instance the arm is shortened by half an inch, measuring from the olecranon to the acromion.

Mr. LYNN differs in opinion from me as to the nature of the injury, and states that it is an accident which he has never seen during the sixty years that he has attended this hospital as student and surgeon. There have been, I believe, two similar cases in this hospital in the course of the last year. One was of a brewer's drayman in Matthew's ward, about forty years of age, who fell on his right shoulder, and suffered the injury I have described, but which I did not then understand as at present. I requested the opinions of Mr. LYNN, Sir ANTHONY CARLISLE, and Mr. WHITE, on this case. Mr. WHITE said then, as now, that it was out of drawing, and Mr. LYNN said the man would get well and have a good useful shoulder, which he had, ultimately, although the projection remained. This case Mr. LYNN recollects, but he does not think that it was quite the same as that of this child.

The second case was that of a boy who lay in Mark's Ward. The late Mr. ALCOCK paid particular attention to him, and the history of the case was taken at length, but the book was lost. This boy had also good use of the arm, although the protuberance was more distinct than in the brewer's arm.

The third case I shall adduce, was that of Colonel Yorke, secretary to Lord Mulgrave, who was about thirty-five years old. Two years ago he was thrown from his horse in Hyde Park. I did not see him until two hours after the accident, and the shoulder was a little swollen, but not so greatly as to prevent an accurate examination. The only symptoms differing from what you see in this child were, a greater degree of protuberance on the inside, and a crepitus on rotating the elbow, which, when present, was sufficiently distinct, but which was occasionally awanting. I made several examinations before I was quite satisfied of the nature of the injury, and Dr. DELPECH of Montpelier, having done me the honour of dining with me after the accident, I mentioned it to him, and as he had never seen a case of the kind, and wished particularly to verify it, I took him to Colonel Yorke's. He was not only satisfied but delighted with the case, and expressed to the Colonel his fullest acquiescence in the opinion I had given as to the nature of the injury. The treatment consisted of a padded back-board, and two broad soft padded straps of chamois leather, which went under the arms, and were fastened to four brass pins on the back-board (a modification of Brasdor's bandage). The elbow was supported, carried forward, and fastened close to his side; and after a time passive exercise of the limb was resorted to. He recovered the use of his arm, and left off his bandages at the end of *ten* weeks, and suffers at this present time no inconvenience from the accident, but there is still a degree of prominence remaining.

The last case I shall bring forward, came under the observation of several of you, although a private patient, because I brought him into this theatre for the more convenient application of the pul-

leys. Mr. Perry the younger, of Perry's-place, Oxford-street, about eighteen years of age, fell on the point of the shoulder at Amersham, and was supposed by Mr. TENNANT, his surgeon, to have dislocated the head of the humerus, which he duly reduced. It did not, however, remain in its situation, and was, I believe, again replaced. Still there was something wrong about it, and at the end of three weeks, the young gentleman was brought to me. On examination I found the elbow was carried very far back, and the head of the os humeri proportionally forward, and apparently luxated in that position; but the prominent part was not round like the head of the bone, and did not give that sensation to the touch; on the contrary, it felt to me as if the head of the bone was split as well as dislocated. I had the pulleys prepared here, and brought him down; the extension was made and kept up for nearly an hour, until, in fact, he could bear it no longer. The bone moved considerably from its original situation on the inner edge of the glenoid cavity under the extension; but it did not long remain in that position when the extending power was removed. As soon as the skin of the arm, which had been abraded by the bandages, had healed, I once more submitted him to the pulleys, exerting even a greater power upon him, whilst in a state of exhaustion from loss of of blood, and from the effects of tartar emetic. I brought the whole head of the bone below the level of the glenoid cavity, but I could not entirely remove the prominence. When taken out of the pulleys, the outer portion of the head of the humerus appeared to be in the glenoid cavity, but the inner and greater part of it was internal to it. This arm assumed, however, more of its natural appearance; the elbow was in its proper direction, and could be readily pressed against the ribs. In order to prevent another escape of the bone, I had recourse to the same apparatus as in Colonel Yorke's case, and treated it altogether in a similar way. He recovered at the end of several weeks, and went into the country with a very useful arm.

In this case the capsular ligament of the shoulder joint must have been torn; and I believe the whole of the head of the humerus was dislocated as well as split, which may be a reason why it so readily became displaced a second, and even a third time; for there is no reason to doubt its having been duly reduced by Mr. TENNANT, at Amersham, although he could not restore the protuberant part to its place, for the reason I have assigned.

I have seen a case in which the humerus was broken, and the head alone remained in the glenoid cavity, but it did not resemble the cases here described. Nothing will effectually clear up the uncertainty which envelops the subject but a dissection of the parts interested it.

REMARKS ON THE FOREGOING CASES,

BY MR. LYNN.

[Mr. LYNN, the senior-surgeon of the hospital, having felt peculiar interest in the case of *Louisa Chapman*, politely requested that the gentleman by whom notes of the lecture were taken for THE LANCET would furnish him with his report previous to its transmission to the Editor. Mr. LYNN, having examined the report, has obligingly forwarded to us the following commentary thereon for publication in THE LANCET.—ED. L.]

My friend and colleague, Mr. GUTHRIE, truly states that I differ from him in opinion as to the nature of the case of Louisa Chapman. When I first saw her the tumefaction of the shoulder was so great, that no accurate opinion of the case could be formed. In a fortnight the swelling subsided, when I distinctly felt two moveable bodies under the deltoid, and which I inferred to be the epiphyses of the acromion and of the humerus. It is to be recollected that the patient is only twelve years old, and that consequently the cartilaginous union of these epiphyses is but feeble, and very likely to yield when a concussive force is applied to the surface of the joint. The girl, the subject of the injury, fell out of a second-story window, and her shoulder pitched on the cover of a water-but. The shoulder, then, came in contact with a flat surface, and the injury was produced by concussion. In that case the weakest parts would give way, which parts are the epiphyses. Mr. GUTHRIE, in his extensive military practice, may have observed many examples of oblique fracture through the head of the humerus, which he designates a *split*, for the penetrating impetus of a sabre, or of a bullet, or of any other of the various modes of operation of compound forces, may produce any form of injury. In the case before us the force was simple, and the consequent mischief such as one might *a priori* have inferred. I have by me a youthful joint which I recently macerated for the purpose of demonstrating to the pupils the points I have alluded to.

An examination of the affected joint as it now exists will afford indications of the true nature of the case. Looking direct

on the glenoid cavity, the joint is broader and flatter. The limb is shortened about an inch. The upward movements of the arm are much impeded, though the patient manages to place her hand on her head. A little in front of the joint, and under the anterior part of the deltoid, is a projection, as described by Mr. GUTHRIE, which is manifestly the end of the humerus. A careful inspection shows the end of this projection to be broad and flat, affording such a surface as the end of the humeral shaft, denuded of its epiphysis, would give. The farther deviation of this bone inwards and forwards is prevented by the coracoid process, and the long head of the biceps with its ligamentous attachments. The projection moves in consent with the shaft of the humerus. A body is felt, both from examination in the axilla and from above, to be in the glenoid cavity, and this body moves also in consent with the humerus. All the movements of the joints are performed, though in some respects imperfectly. What then is the inevitable conclusion, but that the epiphysis of the humerus was in the first instance separated from the main bone, the emancipated head of which was dislocated as far as its attachments would allow?—that an angular or diagonal union has taken place between the shaft and the epiphyses?—that the contact of the articulating surfaces has never been destroyed, nor the integrity of the joint invaded?—and, finally, that the embarrassment of the motions is due to the obliquity of the union I have mentioned? The epiphysis of the acromion being supported in its place by the head of the humerus, was speedily united, as might have been expected.

Of two cases mentioned by Mr. GUTHRIE I have some recollection. The case of the boy I think was analogous to that of the girl just related. The accident of the drayman, who was an adult, forty years of age, and whose bones must have been perfectly consolidated, can scarcely be considered similar to those of the boy and girl. A fracture of the surgical neck of the os brachii affords unambiguous indications of its nature, by the deformity produced. A mere split of the bone, as described by Mr. GUTHRIE, would hardly produce the appearance manifest in the case of Louisa Chapman, for the ligaments would retain the joint entire, and prevent any projection; and if the ligaments were lacerated, much greater deformity would ensue than appeared in any of the cases quoted by Mr. GUTHRIE. For these reasons I am disposed to think that the injury in the adults was a fracture of the anatomical neck of the humerus, where the relations of the separated surfaces would be pretty much the same as in dyalysis of the epiphyses. Mr. GUTHRIE lays much stress upon the opinion of Dr. DELPECH, a French author, who has written on the subject. It appears that this gentleman saw the case of Colonel Yorke one day after dining with Mr. GUTHRIE. I conclude that my worthy colleague, in choosing his opportunity for obtaining the opinion of his distinguished friend, acted upon the generally-admitted principle of "In vino veritas." But wine, although it must be acknowledged to open the heart, may not be so efficacious in sharpening the intellect. It is, however, a singular fact, that Dr. DELPECH is the only surgeon who concurs in Mr. GUTHRIE's views, which views are diametrically opposed to those which, on a former occasion, DELPECH deliberately wrote, printed, and published.* It would be a nice speculation to decide how much of the concurrence is due to the claret (Mr. GUTHRIE's is exceedingly good,) and how much to DELPECH. The cases quoted, however, are highly interesting, because they are rare.

EXPERIMENTS

ON THE

HEAT OF THE BLOOD;

Performed to ascertain the Difference between Blood left to Cool and Blood agitated with Air.

"Développer les phénomènes des opérations chimiques qui s'accomplissent dans le corps vivant, est le but scientifique auquel tendent, en dernière analyse, les efforts de la chimie animale, et fait le but principal de la physiologie, science de la plus haute importance pour l'art de guérir."—*Traité de Chimie, tome septième, par J. J. Berzelius*, 1833.

To the Editor of THE LANCET.

SIR,—The following experiments were performed upwards of three years ago in order to ascertain the difference, if any, between blood left to cool in a flask and blood agitated with atmospheric air in a similar flask. I transcribe the results from a book which I keep at our Infirmary for recording my experiments:—

"Infirmary, 28th June, 1830:—At 20 minutes past 10 a.m. Twenty ounces of venal blood were received in one of my experimenting capped flasks, from an adult male. This blood appeared to be in a normal state. Soon afterwards I took the flask containing the blood into my experimenting room. The thermometer indicated 68° in the shade.

* DELPECH published a case of fracture of the anatomical neck, in his *Chirurgie Clinique*.

"At 30 minutes past 10 o'clock, thermometer	82°
40..... ditto	80°
50..... ditto	75°
At 11 o'clock	73°
10 minutes past 11 o'clock ..	72°
20..... ditto	72°"

In the next page of the above-named journal I find the following record:—

"Infirmary, 13th August, 1830:—At 10 minutes past 2 o'clock p.m., thermometer in the shade at 69°, twenty ounces of venous blood were received in one of my experimenting flasks from a sailor, who complained of headach from excess of blood in the system. This blood appeared to be healthy. I took the flask, with its contents, from the surgery into the room which contains my apparatus, at which time the temperature of the blood was 82°.

The atmospheric air was extracted from the flask by the air-pump, and the blood was agitated *in vacuo*. Fresh air was now admitted and fresh agitation performed:—

"At 20 minutes past 2 o'clock thermometer.....	82°
30.....ditto	82°
40.....ditto	82°
50.....ditto	78°
At 3 o'clock	78°
10 minutes past 3 o'clock ..	77°
20..... ditto	76°
30..... ditto	72°

"Similar agitation was performed every ten minutes. During the performance of the last experiment a heavy shower of rain and hail took place, which lowered the temperature of the thermometer, suspended in the room, three degrees, and caused me to conclude the experiments. I regretted this necessity, as I wished to ascertain the exact time required for agitated blood to arrive at the same temperature with blood in a quiescent state, and from what I observed I think that about double the time would be required till the blood arrived at the 72nd degree.

"These experiments were performed with great caution, such as avoiding to breathe on the flask, and holding it in such a manner, with gloves on the hands, as that little or no heat was communicated from the operator to the blood in the flask."

These experiments show, most unequivocally, that in the process of respiration, and consequent conversion of the free carbon of the blood, and the oxygen of the atmospheric air, into carbonic acid gas, heat is given out, which is very properly called "animal heat."

Compare these experiments with those performed by me, and recorded in the pages of THE LANCET for last year, in which the proportions of the free carbon in arterial and venous blood are accurately investigated.

From these experiments we know that the formation of animal heat is a chemical process, and not dependent upon the action of the brain and nerves, as conjectured by several eminent pathologists.

I attach much importance to these experiments, as they were performed long before I had made any efforts (during the invasion of epidemic cholera) in order to ascertain whether such a substance as "free carbon" entered into the composition of blood.

I did not at that time, viz. the year 1830, examine the tint of blood during the course of those experiments, which I have since proved to take place, when the free carbon is under the process of removal by the agency of the oxygen of the atmospheric air, as I did not then attach any value to the change of tint in blood, having perused attentively the details of experiments performed by Professor Brande and other eminent chemists upon the colouring matter of that fluid, by which I knew that different gases, as well as solutions of several neutral salts, gave a variety of tints to the blood, without any chemical change being thereby inferred.

I remain, Mr. Editor,
Your obedient, humble servant,
W. REID CLANNY.

Sunderland.

CASE OF

UTERINE HEMORRHAGE,

IN WHICH THE OPERATION OF

TRANSFUSION

WAS SUCCESSFULLY PERFORMED.

By CHARLES WALLER, *M.D.*

I FORWARD for insertion in THE LANCET the details of a case which has lately occurred, in which transfusion of blood was employed with the happiest result.

On the 18th of December, about half-past one, I was requested to visit a patient thought to be sinking from uterine hemorrhage; she was about eight months gone with child, and had no distinct labour pains. Mr. Greaves, the gentleman in attendance, informed me that she had been losing blood in large quantities for several days, that he had this day examined, and found it to be a partial presentation of the placenta; on introducing his hand for the

purpose of turning the child, the patient was seized with such death-like faintness, that he thought it prudent to desist, and I was desired to meet him in consultation. He had been exhibiting stimuli from time to time without the least apparent benefit.

The countenance of the patient when first seen by me was completely blanched, not the least appearance of redness being observable in the cheeks or lips, the extremities cold, the breathing very laborious, the pulse excessively feeble, the whole surface of the body was cool, and the skin had a soft yielding feel, and indeed her general appearance was that of a woman sinking from exhaustion. A quantity of ardent spirit was again given her, but it failed to excite even a temporary rally.

The question naturally arose, What is to be done? and the usual answer would have been, to deliver immediately, taking every precaution in our power to secure the patient from hemorrhage afterwards. But the objections to this plan were so great, that we did not think ourselves justified in making the attempt:—the female was lying in a condition which rendered it doubtful whether she would rally at all, and certainly in that state wherein a further loss of blood must have at once destroyed her. The vagina was now filled with coagula, and the circulation so low that the hemorrhage had entirely ceased. The introduction of the hand must of necessity have disturbed the clots, and have unstopped the orifices of the bleeding vessels, and thus renewed the hemorrhage; and, indeed, my deliberate judgment is, that the mere act of delivery, even unattended by any further effusion of blood, would have caused immediate dissolution.

After waiting for some time, and repeating the stimuli, combined with nourishment, we found our patient getting weaker, and it appeared to us, that transfusion offered itself as our only resource. The question as to the propriety of emptying the uterus first was then agitated, but for the reasons just mentioned, we thought it better to endeavour to revive her by at once performing the operation, watching very narrowly that the hemorrhage did not come upon us unawares, determining not to interfere at all unless the bleeding was renewed.

The operation was performed in the usual manner, rather more than four ounces of blood having been injected, which was supplied by a female in attendance. The patient then appeared greatly revived, and as the blood flowed very sluggishly from the arm of the person supplying us, we agreed to inject no more at that time. Small quantities of nourishment with stimuli were occasionally given, and for about two hours we seemed to maintain the advantage we had gained, but in a short time subsequently, the symptoms of exhaustion returned, attended with jactitation.

I gave the patient at this time a little gruel with about an ounce of brandy in it, but although it was retained on the stomach, no improvement was observed, but rather the reverse, and it was therefore agreed once more to transfuse. The first four ounces of blood were taken from the individual who furnished it before, but no good effect was noticed; and as she bled very slowly, I requested her husband to allow one of his veins to be opened, the blood from which flowed in a very impetuous stream. After five ounces had been thrown in (in addition to the previous four ounces taken from the woman), the rally seemed perfect, and there was even a slight tinge in the capillary vessels of the cheek, but as her husband was by no means faint, and as we were anxious to avoid the necessity of again operating, three ounces more were transfused, making altogether rather more than twelve ounces at the *second*, and between four and five ounces at the first injection.

There was from this time no relapse; the patient took nourishment frequently, given in small quantities, and complained of nothing but a feeling of extreme fatigue; she had during the evening some grinding pains; about five in the morning the uterus was beginning to act well, and Mr. G., who was with her, informed me that she was safely delivered of a dead child about six o'clock, there being no recurrence of hemorrhage.

She remained very comfortable for the first three days, but on my visit to-day (23rd) she complained much of her head, attended with great thirst. She had an enema administered on the 21st, which procured a very satisfactory alvine dejection; this was repeated yesterday (22nd), but came away without motion, on which account Mr. G. ordered half an ounce of castor oil to be taken, which purged her violently; her tongue to-day is dry and glassy, and she has had but little rest. Utatur Capiti *Lotio Evaporans*, et cap. 4tis horis, *Mist. Salin.* cum *Tr. Opii* ℥ vij.

Bartholomew Close,
Dec. 28th, 1833.

OINTMENT OF VERATRIA

IN DROPSY, TIC-DOULOUREUX, AND RHEUMATISM.

To the Editor of THE LANCET.

SIR,—As an advertisement has already appeared of a work which I have now in the press, on the remarkable medicinal effects of *veratria* and its compounds when applied externally, it would have been unnecessary to send you the present communication, but for the circumstance that numerous prescriptions, containing the medicine, and prepared according to my method, have been recently, and are now, in use in the profession. In answer to the numerous inquiries which have been made on the subject, and to guard the new remedy as much as possible from falling into disrepute through improper application, I have in the mean time taken the liberty of communicating, through the medium of your widely-circulated and highly useful journal, the following information regarding it.

It is now upwards of four years since I was led from certain circumstances to apply veratria, externally, in the form of an ointment, and the instance in which it was employed was one of the most formidable cases of general dropsy ever observed. It had resisted every medicine hitherto deemed of service in that disease; and it was not until death appeared inevitable, that the veratria was had recourse to. An ointment consisting of four grains of the alcaloid, reduced to fine powder, and an ounce of hog's lard, was directed to be rubbed, night and morning, over the abdomen. In about fourteen days the patient was completely cured, and has since continued in perfect health. A particular account of this case will be given in another form. Since the date of the first experiment I have made many others, and always with equal success. Lately, the practice has been extended to the treatment of diseases of a very opposite nature, with the greatest benefit to the patients. In tic-douloureux and rheumatism, one or two applications generally suffice to remove the paroxysm, and should the symptoms return, they may again be removed with equal ease. A similar treatment is of the most material service in various affections of the heart and circulating system. For these and numerous other diseases the ointment is made with from fifteen to twenty grains of veratria to an ounce of lard, of which a piece, the size of a nut, is rubbed, night and morning, from twelve to fifteen minutes each time (as nearly as possible), over the seat of the disease, until relief from the urgent symptoms is experienced. There are certain affections, particularly encysted dropsies, in which the ointment requires to be used for a considerable length of time before a marked change takes place; but as the various conditions to be observed in such cases, along with the collateral treatment to be employed, would occupy more space than it is intended at present to devote to the subject, the inquirer is referred to the publication itself for all necessary information.

I am, Sir, your obedient servant,

A. TURNBULL, M.D.

Russell-square, Dec. 16th, 1833.

ON A

NEW SCARIFICATOR

AND

THE OPERATION OF CUPPING.

By EDWARD THOMPSON, *Esq., Whitehaven, M.R.C.S.L. &c.*

Mitti vero sanguis debet si totius corporis causa sit, ex brachio; si partis alicujus, ex ea ipsa parte.—CELSUS, Lib. II.

IN a late Number of the *Dublin Medical Journal*, Dr. Osborne has given a description of an instrument which he calls a polytome, intended to supply the place of the more expensive and complex one commonly employed in the operation of cupping. With a slight difference as to shape, and the want of a sliding spring to regulate the depth of the phlemes, it is similar in every respect to one which I have for several years been in the habit of employing, and which was made from a model given by me to Mr. Brown, surgical instrument maker, of Newcastle. The small quantity of blood obtained by the common scarificator, was one cause of my resorting to lancets fixed in a handle, which constitutes the instrument now under discussion. The extreme simplicity of the instrument prevented me giving a detailed account of it in any medical journal previously; but now that another has introduced it to the consideration of the profession, I think it right to participate in the *honour* of the discovery. In the mode generally adopted of taking blood by cupping, a degree of dexterity is required, which some never attain. The scarificator is applied and reapplied, without more than a few ounces of blood being obtained, and even with the most dexterous more

blood is never expected to be taken than about five ounces by each glass. To take thirty ounces, therefore, requires six glasses, which, by the method I have long adopted, might be effected by two scarifications. In the area of six inches the new scarificator gives us eight inches of incision, whereas the old one of ten lancets cannot be effectively made to extend to more than three inches; and when it is remembered, that with the former the operation can be performed with more rapidity than, and with the infliction of as little pain as, if rightly executed, the latter, the expense of time and trouble may be well imagined to be much lessened, and other advantages gained which the usual plan of proceeding does not afford.

An object of importance in the performance of the operation, is to have the blood as quickly as possible removed from the surface acted on by the glass. In what has been deemed a rude method practised in Zetland, this, perhaps without intention, has, in part, been pursued for ages. The gourd, or horn, of necessity allows the blood to flow far from the cut surface, and the proportion therefore is greater than can be obtained by the cupping-glass in general use. The method of proceeding in that country not being well known, and a few of the steps deserving attention, the following extract from an essay on rheumatism, by my friend Dr. Copland, the justly-celebrated and learned author of the Medical Dictionary, may be not without its value. "Quam partem volunt scarificare, hanc aqua calida fovent. Qui medici partes agit, is cutem sexies aut septies novacula per quam leviter perstringit, et cornu aritineum modice recurvum, quod cucurbitulæ vice fungitur, apice perforato, et corio molli circumdato, partem leviter resectam applicat. Tunc foramini labia admovet, et quantum fieri poterit, aera inclusum exsugit. Quum cornu exinanisset, corio torquando, et in foramen lingua protrudendo aeris irruentis impetum prohibit. Postquam cornu partem scarificatam arripit, deinde pannos ex aqua calida paulum exsiccatos circa unum cornu superimponit, qui sanguinem ad partem provocent. Quem sanguinis semiplenum sit, cornu tum cutem relinquit et decidit. Eadem res iterum et iterum repetitur donec satis sanguinis mittatur."

The glass which I have used is the one recommended by Mr. Gordon of Cork, with the exception of the stop-cock. Fox's glass-leech is a copy of it, taken without acknowledgment. With this glass, and the new scarificator, any quantity of blood may be taken that the operator wishes, without any particular dexterity being required, farther than mere quickness in effecting the incisions. In my instrument an error was originally committed in placing the phlemes too near each other, and in this way limiting the supply of blood from the vessels lying between the incisions, and also in making the phlemes too long. The addition of a sliding spring is, I am satisfied, from frequent trial, one not to be overlooked, particularly when operating on children, and as it adds but little to the expense of the instrument, ought not to be omitted. The whole cost of the instrument does not amount to more than fifteen shillings, which is another recommendation among the many it possesses over the old scarificator.

Whitehaven, Nov. 1833.

PRESERVATION OF LEECHES.

To the Editor of THE LANCET.

SIR,—I have given Lieutenant Bathe's plan, as communicated by M. Moreau, of Angers, a fair trial, and consider it, although not more natural, still far superior to the common mode of preserving leeches in jars among water. I found, however, that leeches kept for some time in earth, though they continued to live, became dull and heavy without a constant supply of water at their side. I therefore beg to recommend to the notice of the profession a plan which I have found eminently successful. In October, 1832, I procured a box, water-tight, two feet long by fourteen inches wide, and eleven inches deep, with a lid and hinges, which fitted so closely as to prevent the escape of the smallest leech. In the middle of the lid a perforated tin-plate was inserted to admit a supply of air, and in one end of the bottom of the box was a small hole, for the purpose of permitting the water to be discharged when requisite, and which otherwise was closed by a cork. In the opposite end of the box was a bank of common earth (which will answer better if it be of a clayey nature), about six inches deep, and covering one-third of the surface of the bottom. The water in the remaining part of the box should be four inches and a half in depth, or one inch and a half from the surface of the earth. When the box is thus prepared for their reception, the leeches may be put into the water, and they will repair to the earth at pleasure. A fresh supply of water must be afforded every week, at which time the bank of earth must be watered. In November, 1832, I put 200 leeches into the

box amongst the earth and water, and out of this number I have only lost eight in the course of eleven months. Sometimes the whole of them would be among the earth, while at another time most of them would play among the water.

I have never seen any ova or young leeches, but from my own success in this experiment I have reason to believe that should this plan be generally adopted, a material reduction in the price of leeches would be the inevitable consequence.

I am, Sir, your obedient servant,

E. B. SHERRIFFS.

Brechin, Nov, 1833.

TREATMENT OF

UTERINE PHLEBITIS.

To the Editor of THE LANCET.

SIR,—Will you permit me, through the medium of your journal, to correct an inaccuracy into which "An Observer," at the Middlesex Hospital, has fallen in his account of cases illustrating a presumed connexion between disease of the uterus and inflammation of the veins of the thigh, which appeared in THE LANCET of Dec. 14, p. 448. "I would refer your readers (says he) to some interesting cases published by Dr. Lee, in which there occurred inflammation of the veins of the uterus after parturition, causing puerperal fever. They were treated upon the depletory system, and all died, when the same results were found after death which, no doubt, would have been noticed in the subjoined cases had they not survived the acute attack. With regard to the treatment of phlebitis, (adds "An Observer,") whether connected or not with uterine disturbance, I should say that venesection must never be resorted to; though we are not to watch the progress of this destructive malady, and be content to apply simple remedies. We are in possession of one which will at all times be found to grapple with the disease, and which, by exerting its peculiar influence on the system, will arrest the inflammation, and prevent those depositions in the veins which will ultimately render them impervious, and give rise to a long train of distressing symptoms. This remedy is calomel, given in frequent doses, so that its effect may be rapid and sudden; and with it alone, I firmly believe, far more may be accomplished than accrues from general or local blood-letting, though the latter is of more value than the former."—These observations induced me to consult the work of Dr. Robert Lee on the pathology and treatment of some of the most important diseases of women, to ascertain whether the preceding statements were correct, and I was astonished to find that in most of the cases of uterine phlebitis recorded by Dr. Lee depletion was not had recourse to at all, and in no instance to a great extent. In the following passage it will be seen that he has recommended the opposite plan of treatment, and that calomel, which is so highly extolled by "An Observer" as a certain remedy in phlebitis, has been tried and has failed, like all other remedies:—

"With regard to the treatment of inflammation of the uterine appendages, and of the deeper-seated tissues of the uterus itself, whether of the absorbents, veins, or muscular structure, the symptoms from the commencement are generally those which contra-indicate the use of general blood-letting. In cases when the reaction at the invasion of the disease has been violent, and venesection has been employed, the relief obtained has only been temporary, and in some instances the abstraction of a small quantity of blood from the arm has produced alarming syncope. In many cases the blood will not flow in a stream when venesection has been performed, a few drops only trickling down the arm. When the local pain is severe, leeches and warm fomentations seem to be the most appropriate remedies; but as far as my own observations extend, we are not at present in possession of any remedial means which effectually control those varieties of inflammation of the deeper-seated structures of the uterus, which I have endeavoured to describe. The French physicians are however of a contrary opinion, and are satisfied that we possess a powerful remedy, even in the worst cases, in mercury, employed so as to excite salivation. In several cases of uterine phlebitis I have employed this remedy to a great extent, externally, and speedily brought the system under its influence; yet the progress of the symptoms was not arrested, and the patients died, as others had done, when the mercury had not been exhibited. In other cases I have employed mercury to a great extent, internally, without the slightest benefit; and it may justly be doubted from the results of M. Desormeau's practice, whether or not it possesses the influence M. Tonellé supposes, for of forty-three cases, where mercury was used by him as the chief remedy, only fourteen recovered. In the latter stages of inflammation of the deep-seated structures of the uterus, the great depression of the powers of the system renders the liberal administration of stimulants absolutely necessary, and

in several cases of phlebitis the life of the patient appeared to be preserved by them." Page 112.

If an observer will consult other parts of this work, as I have been led to do in consequence of his observations on phlebitis, he will find many facts respecting phlegmasia dolens in men, and in women who are not in the puerperal state, which may assist him in his inquiries in the important subject on which he has written. I have the honour to remain, Sir, your obedient servant,

AN OLD OBSERVER.

London, Dec. 16th, 1833.

THE LANCET.

London, Saturday, December 28, 1833.

NOTHING can exceed the dismay of the BATS. Poor devils! They are already frightened out of their senses at the consequences which are sure to result from the success of one of their own manœuvres. These creatures are surely the most imbecile of all living things! Is it possible, however, that they were so thoroughly stupid as to believe that medical practitioners would sit down quietly under the promulgation of the atrocious calumny, that they were sufficiently indifferent to the fate of medical reform as not to desire the institution of THREE GREAT FACULTIES OF MEDICINE in the three great departments of the British empire? Where do these blind and senseless creatures congregate? Where do they maintain their anomalous existence? Do they never move except in twilight, and have they the folly to believe that darkness will always prove an impenetrable cloak for the protection of impostors? Ah! that title of "DOCTOR!" How they hug and cling to that deceitful emblem,—through the instrumentality of which they have so long been enabled to insult the great mass of general practitioners in medicine, and to humbug the public! But to return to the debates.

Dr. GEORGE GREGORY,—what university claims the distinguished honour of having made this man an M.D.?—Dr. GEORGE GREGORY, "would not have attended the discussions at all, if he had thought that the existing medical colleges and companies were to be abolished. It was *his* desire to *reform* the existing institutions." Well; it was a benevolent and a reasonable desire. But why did not Dr. GEORGE GREGORY condescend to explain to the members of the Society and to the profession at large, what change in the construction of those institutions *he* would consider as essential to the *reformation* which he contemplated? The omission was ungenerous, because, to a man of his enlarged capacity, an exposition of noble views on the subject of medical polity could not have proved a heavy tax on *his* intellect.

The advocates of one great FACULTY OF MEDICINE, divided into three working departments, behave not thus coyly or niggardly towards their opponents. Influenced by just motives, stimulated in their conduct by sound principles, they boldly and unhesitatingly promulgate a scheme, the operation of which they are unequivocally of opinion would consolidate the interests of the profession, and by promoting the health, necessarily increase the happiness of the community. Objecting to the *sixteen* manufactories now existing in England, Ireland, and Scotland, in which candidates for medical honours are converted into physicians, surgeons, and apothecaries, "human and *divine*," the advocates of a Faculty reject such a massive, incongruous, ill-working machine, and stoutly contend for the propriety of constructing an instrument, in the operation of which there shall be no jarring, eccentric, or opposing, movements. The opponents of the Faculty admit, that such a contrivance is a desideratum, yet in the same breath clamour for a "reform" of exist-

ing institutions, and attempt not, even by inference, to show that the construction of *one* Faculty, in three parts, would prove a labour of difficult execution.

What, then, we inquire, is to constitute the "reform" for which Dr. GEORGE GREGORY and the rest of his *clique* so noisily contend? "Oh," exclaim those wiseacres, "root out the *abuses* in our "medical colleges, let us not abolish the "*institutions*;—give them time to reform," —thus presuming that the existing corporations command the power to consummate their own reformation. What a pity that a man possessing the enlarged intellect of Dr. GEORGE GREGORY should overlook the trifling fact that the "existing institutions" are founded on WRONG PRINCIPLES,—that they are one and all defective in law,—and that what he and his brother conjurors denominate, or consider to be, "abuses," springing out of the *maladministration* of the law by the heads of those institutions, have been the natural, the inevitable, results of the ill-constructed statutes? The heads of the Colleges and Companies have not been so guilty in violating the law, as they have been unfortunate in becoming the agents of its execution, and yet a senseless cry is raised for "reform,"—as though effecting a thorough change in the *principles* and *working* of a charter did not virtually amount to the *abrogation* of that charter.

But we are forgetful. Dr. GEORGE GREGORY promotes that description of "reform" which will convert *three* institutions into—*three* institutions! That is, he is most desirous that there should exist in London a College of Physicians, a College of Surgeons, and a Company of Apothecaries. "It is," he alleges, "for the *good* "*of the profession* that they should con- "tinue to exist." Good for Dubs, GEORGE, you mean. What, for example, is GEORGE GREGORY, if he be not a "Doctor," and what would be GEORGE GREGORY, if all really-scientific practitioners were, like himself, entitled to the appellation of "Doctor?" Verily, GEORGE, the instinct of self-preservation in thee is somewhat acute.

How, then, would this graduate of some (to us) unknown university set about *reforming* the medical corporations of this metropolis? Why he would necessarily first direct his benevolent views to the Royal College of Physicians in Pall-Mall East. We can almost fancy that we hear him addressing the eel-backed baronet thus:—

GREGORY.—"Your very humble ser- "vant, Sir HENRY. As you are always in "the secrets of the great council of the "nation, you are doubtless already well "informed of the object of my visit. From "the success of my little manœuvre at "the Westminster Medical Society, some "Tory leaders have exercised sufficient "influence over the Whig Ministry to "make them promise to institute such a "reform of the College of Physicians as "*you* may comply with."

HALFORD.—"I feel deeply honoured by "the confidence thus reposed in me by "his Majesty's Government. Pray, Sir, "what is the extent of reform that you "solicit?"

GREGORY.—"Why, Sir HENRY, we "reformers consider that the College of "Physicians would form at once the most "powerful, the most learned, the most "useful, and the most perfect, institution "in Europe, if the *licentiates* of the Col- "lege,—graduates in medicine of the "University of Edinburgh—of which body "I have myself the honour to be one,— "were to be styled 'Fellows' instead of "'Licentiates,' and were to be allowed to "participate in those powers of the College "which are now exercised exclusively by "the graduates in medicine of the Uni- "versities of Oxford, Cambridge, and "Dublin."

HALFORD.—"Is this all, Sir, that you "require for the College of Physicians?"

GREGORY.—"All, all, Sir HENRY."

HALFORD.—"What alteration, Sir, are "you desirous of promoting in the Col- "lege of Surgeons?"

GREGORY.—"Why, Sir HENRY, I am "not very particular with respect to that "College. I think the surgeons have al- "ready encroached too much on the prac- "tice of the physicians. If any change "in the law, however, is to be made rela- "tive to that institution, I should like "to see its members restricted in their "practice to manual operations,—such as "the setting of fractured bones, the cut- "ting for stone, the application of ban- "dages; and that the penalties of the "Apothecaries Company should always "be enforced against them, whenever "they presume to hazard the health of "the community, by prescribing for the "cure of internal diseases,—a knowledge "of which calamities—I say it, Sir, with "confidence—can only be known to the "graduates in medicine of the universities "of Oxford, Cambridge, and Edinburgh."

HALFORD.—"Well, Sir, what reform "do you require in the Act of the Apo- "thecaries Company?"

GREGORY.—"Why, Sir HENRY, I only "claim that apothecaries should be phar- "macopolists merely, and that the licen- "tiates of that Company, like the mem- "bers of the College of Surgeons, should "be subjected to very heavy penalties "when they dare to prescribe for the "sick."

HALFORD.—"Then, Sir, the changes "you require include the admission to "the Fellowship, of the Licentiates of the "College,—Graduates of the Universities "of Edinburgh; and the imposition of "heavy penalties on members of the Col- "lege of Surgeons, and Licentiates of the "Company of Apothecaries, whenever "those unqualified pretenders dare to in- "vade the province of the Fellows of the "College of Physicians—the only physi- "cians in England 'ostensibly' qualified "to prescribe for the sick in medical "cases."

GREGORY.—"Exactly, Sir HENRY; ex- "actly so."

HALFORD.—"There is only one objec- "tion to this scheme, Dr. GREGORY."

GREGORY.—"Pray explain it, Sir HENRY."

HALFORD.—"It won't do. Good morn- ing, Dr. GEORGE GREGORY."

It is scandalous that we should be thus put to the torture of stating GREGORY's views for him, but if his admiration of some such scheme as we have here ex- plained, be not urging him to call for "re- form" in obscure and undefined terms, why does he not explain, with boldness and honesty, those changes of the law which he deems essential to the intro- duction of a healthy system of medical polity? Unfortunately, however, the GRE- GORIANS, instead of making candid state- ments with respect to the alterations which they consider desirable, treat the question of medical reform as a piece of patch-work, and select only one or two spots in the chequered material whereon to found their conclusions.

It cannot have escaped observation, that while the Licentiates of Apothecaries Hall are denouncing that Company,—that while the members of the College of Sur- geons are condemning the Government of that College,—the Graduates in medicine express, in very soft accents, their disap- probation of the proceedings of the insti- tution in Lincoln's-Inn-fields, and loudly proclaim the benefits which have been conferred on the profession, by the Wor- shipful Society of Apothecaries. The Gra- duates of medicine practising in London, Licentiates and Non-Licentiates of the College of Physicians, vent the torrent of their indignation against the institution in

Pall Mall East—merely the spray of the current of their displeasure falling on the other two incorporated medical companies. This circumstance sufficiently proves that no man can be competent to discuss the question of medical reform, until he has taken a comprehensive and an impartial view of the whole question. Notwithstanding the anathemas of the Graduates, the College of Physicians is really a liberal, pure, noble institution, when compared with those receptacles of tyranny and and avarice, the College of Surgeons the Company of Apothecaries. Nor, considering the *charter* of the College, are the governors of that institution really illiberal in carrying into practice the forms of law which regulate their proceedings. Let us examine the conduct of these several bodies:—

The College of Surgeons in Lincoln's-Inn-fields rejects nearly all the testimonials which are presented by candidates for its diploma, unless those testimonials emanate from the members of the council or their relatives. A candidate is refused an examination for the diploma of that institution, unless he produce proof of his having been engaged at least six years in the acquisition of "sound chirurgical knowledge"; he must produce certificates of having attended certain courses of anatomy in the winter season; certificates of attendance on lectures on surgery, the practice of medicine, materia medica, chemistry, botany, midwifery, &c.; and, finally, of having attended the surgical practice of a hospital in London, containing one hundred beds, during one year; or during two years in a hospital in the country, and an attendance of six months at one of the London institutions. Having complied with these demands he is admitted to an examination,—a precious boon, truly,—when, having paid his twenty-two guineas for the privilege of being egregiously humbugged, he is styled "a member of the College," and henceforth belongs to the "COMMONALTY" of the institution—the College itself consisting, *as explained in the charter*, of president, council, and COMMONALTY. What are the prerogatives of the new member? He is entitled to have his head broken by the bludgeons of hired ruffians, at the command of the council, if he at any time presume to stand up in the theatre of his own College for the maintenance of the privileges of his brother members of the commonalty. He is entitled also to the right of being proceeded against by criminal information for the same heavy offence; and he has the further satisfaction of knowing that all the honorary and emolumentary officers of the institution, are conferred by a self-perpetuating junto, on, frequently, the least deserving members of the commonalty!

Such is the college which has only produced a passing condemnation from some of those graduates of medicine, who are, nevertheless, sincere in their support of the cause of medical reform. The Apothecaries Company not only demands from candidates the production of a long list of certificates, rather proving *payment* for, than *attendance* on, lectures, and hospital or dispensary practice, but it also requires evidence of having served a five years' apprenticeship; and not only refuses to examine graduates in medicine of our chief universities, fellows and members of the best of our medical colleges, but proceeds to prosecute, as unqualified pretenders, gentlemen holding diplomas from the Universities of Dublin and Edinburgh, from the Colleges of Surgeons of those cities, and also from members of the College of Surgeons in London. Yet this vile, trading, persecuting, Company, is lauded by those very graduates in medicine, who condemn, as the most illiberal scandal of the age, the Royal College of Physicians in London.

What then is the discipline of the latter institution, that it should form the focus of the concentrated indignation of all the graduates in medicine practising in London? The answer is obvious. It is because the College of Physicians will not admit the Licentiates to the full privileges enjoyed by the Fellows,—to participate in electing the officers of the institution,—to share in the government of the College. Pray are the *Licentiates* of the *Apothecaries Company* permitted to vote in electing the members of the court of that body? Are the *Licentiates* of *that* Company permitted to take a part in the government of the corporation? Nay, more; do all the members of the *College of Surgeons*, who, according to the charter, constitute, *bona fide*, a part of the College, and amount to upwards of six thousand in number—do *they* exercise the least, the remotest, influence over the elections which take place in the College in the appointment of members of the council?

What, on the other hand, is the every-day conduct of the *College of Physicians*, with respect to the admission of licentiates? Do the President and Censors of that institution insist on the production, by candidates, of innumerable certificates,—of indentures of apprenticeship,—of attestations of years of hospital attendance? No; on the contrary, with a degree of liberality unequalled in the observances of medical colleges and corporations, this College—the most reprobated of all the medical institutions,—admits without demur or hesitation, not only all graduates in medicine who belong to the English, Irish, and Scotch universities, but they are ever ready and willing to recognise the diplomas of French and German universities, as establishing the eligibility of candidates to an examination for their licenses. Fifty English and foreign diplomas would not procure an admission into the porter's lodge of the Worshipful Company, or into the den of the Secretary of the College of Surgeons!

But this is not all, for the Apothecaries Company, that just and liberal institution, which has "effected so much good" forsooth, pounces on the University graduate, and prosecutes him as an unqualified practitioner, although he may be actually in possession of the license of the College of Physicians. In a word, the College of Physicians is vehemently condemned by the mock-reformers for prosecuting unlicensed physicians, while the Company of Apothecaries is applauded, by the very same parties, for prosecuting *licensed* physicians.

Such are the partial views which individuals who examine only one branch of a complicated question are prone to take. We should endeavour, as far as it is possible, in all important discussions, to enter upon the inquiry without reference to our own immediate interests. If the Gregorians had acted on this useful rule, they would have discovered the impolicy of allowing the present distinctions in the profession to continue, by maintaining, intact, the Colleges and Companies which are now devoted to what are called "the three branches of the profession."

If, however, it be the determined object of certain physicians to uphold, by every available stratagem, the absurd and degrading distinctions of *title*, which exalt the ignorant at the expense of the scientific and learned portion of the profession, it is satisfactory to find that there are, nevertheless, physicians of eminence who make common cause with the mass of English, Irish, and Scotch surgeons, in insisting on the propriety of adopting one uniform title for all legally-qualified medical practitioners, and we cannot in this place refrain from expressing our warm admiration of the bold and zealous conduct of Dr. James Johnson and Dr. James Somerville, who are licentiates of the College of Physicians,—by the latter

of whom the question of medical reform was first brought under the notice of the *Westminster Medical Society*, and, by the former, the motion for the institution of a FACULTY OF MEDICINE was first proposed in that assembly. If, then, there be a party of physicians, whose object it is to degrade and insult the majority of English surgeons, there are to be found physicians of character and influence who stoutly contend for the institution of a FACULTY OF MEDICINE, in which a principle of justice shall prevail in bestowing the title of "Doctor."

When speaking in the last LANCET of the paltry manœuvre of the GREGORIANS, we stated that the mask was now thrown off, and that the mock-reformers, if not by words, had, at least by deeds, made manifest their disgusting intentions; but we certainly were not prepared for the open opposition which has been displayed by certain hangers-on of the medical Colleges to an efficient renovation of our medical system. We anticipated that the hostility of the corruptionists would have been carried on covertly. Certainly we did not expect that any number of them were sufficiently intrepid to declare WAR against the surgeons of the British empire, and impudently proclaim that the mass of the profession consist of a set of ignorant and unqualified practitioners! What, however, was the language of the hired scribbler of the BATS? What was the language promulgated by MACLEOD, the HACK of LONGMAN and CO, in the last number of his masters' Excrescence? We entreat that the reader will pay particular attention to these words, because they are written by COMMAND OF THE RAPACIOUS DOLTS WHO BELIEVE THAT IT IS YET IN THEIR POWER TO RESIST THE INFLUENCE OF PUBLIC OPINION:—What is the language held in the Excrescence of last week? why that "out of upwards of three thou-"sand two hundred licentiate Physicians, "Surgeons, and Apothecaries, ALL EN-"GAGED IN THE PRACTICE OF "PHYSIC IN LONDON, TWO HUN-"DRED ONLY ARE OSTENSIBLY "QUALIFIED BY APPROPRIATE "STUDIES AND EXAMINATIONS. "The two hundred who, only, are osten-"sibly qualified to practise by appropriate "studies and examinations, are THE LI-"CENTIATE PHYSICIANS."

Mark this language, surgeons and apothecaries of this metropolis! You are here denounced to the public by this reptile of a writer, as incapable of discharging the duties of your profession. If the BATS can carry on the war, in this daring manner, against the undoubted rights of the mass of British practitioners, we shall give them credit for valour, at least, whatever may be our opinion of their discretion and love of justice.

But the announcement of the intentions of the insolent corruptionists is assuredly premature. It will be seen by our report of the proceedings of the *Westminster Medical Society* on Saturday evening last, *that the cheer has* PRECEDED *the victory*. The ballot question has not reached a final settlement,—Dr. EPPS, with characteristic zeal in the cause, having given notice of a motion, the result of the discussion on which may prove that the manœuvre of the GREGORIANS, although ingenious, was opposed to the spirit and letter of the laws of the Society.

But the great source of alarm to the hole-and-corner gentry is to be found in the announcement of an AGGREGATE MEETING OF THE PROFESSION, at which assembly, if we mistake not, the discomfited GREGORIANS will receive a final sentence of condemnation.

Facts establishing the Deleterious Properties of Rice, used as an Article of Food. By ROBERT TYTLER, M.D., Surgeon in the Honourable East India Company's Service, Chief Surgeon of Fort Marlborough and its Dependencies, Acting Superintending Surgeon of the South-Eastern Division of the Bengal Army during the Campaign in Arracan in 1825, Acting Superintending Surgeon of the Dinapore Division of the Bengal Army, and Senior Surgeon in the Field during the Campaigns of 1832, 1833, against the Coles and Choars in Chota Nagpore, and the Jungle Mehuls. London: Renshaw and Rush, 1833. 8vo. pp. 60.

THE recent discussions on the rice question in the *Medical Society of London*, were of far too interesting a character not to have excited in the minds of medical practitioners generally, a feeling of the vast importance of the subject debated. The great majority of experienced physicians are so reluctant to branch off into any new course out of their old beaten track of observation, that discoverers usually experience considerable difficulty, in directing the serious attention of medical writers and practitioners to any newly-ascertained fact, whether it relate to diet or disease. Nor is this extraordinary, when we reflect on the theories, founded on alleged facts, which have flourished for a time, and then, having been falsified by experience, have fallen into oblivion, or have been cast aside by one generation, to be brought forward again only that they may amuse or deceive another. It is seldom, however, that the hypotheses put forward by authors and practitioners have been distinct from those considerations and circumstances which are pleasing to the personal vanity of their propounders. Thus, a new theory of disease is commonly the precursor of a new remedy, and the "discoverer" of the peculiar malady is, of course, the "discoverer" also of the means by which it can be cured.

In the pamphlet before us, there is a striking deviation from the rule just cited. Dr. Tytler presents us with an immense number of FACTS in proof, that life is most prodigally sacrificed by the use of an unwholesome article of diet, but he boasts of no *cure* for the malady which the vegetable poison engenders. A precautionary system, however, naturally arises, out of the strikingly important discovery of Dr. Tytler, which must prove far more beneficial in its results than could any curative agent, however potent, searching, or successful.

The opinions of Dr. Tytler concerning the deleterious properties of rice used as an article of food when the grain is in an unsound state, and the more prominent facts on which he has founded them, have been presented to the profession in such an ample form in our reports of the proceedings of the *London Medical Society*, that it only remains for us on this occasion to make our readers acquainted with the arrangement adopted in the pamphlet before us, referring them to the production itself for such additional references relative to time and circumstance as did not appear in the course of the debates. The pamphlet is divided into twelve sections. In the *first*, Dr. Tytler shows that his opinions of the deleterious properties of rice do not originate with him. His researches have disclosed, that more than seventy years since bad rice was detected as a cause of violent morbific ations in the animal frame. Dr. Tytler had been a resident in India ten years before the appearance of the cholera in Jessore, and in the course of his literary pursuits had met with passages in which rice was described as occasionally proving an injurious article of food. Their effect on his mind, however, was slight, until he heard from a native, in reply to his inquiries respecting the first cases at Jessore in 1817, that "new rice made all those sick who ate it." Suddenly the remarks of deceased authors flashed upon his memory, and from that time facts fast accumulated confirmatory of his opinion, that there is such a disease as *morbus oryzeus*, and that the so-styled "cholera" of India is chiefly that disease.

In the *second* section Dr. Tytler presents the reader with facts which have been ascertained subsequently to 1817. In the *third* and *fourth* sections, he produces well-attested experiments of the

destructive power of rice upon domestic animals. As the facts here disclosed were but very briefly referred to in our report from the Medical Society, we shall transfer to our pages some passages from this section. The attestator is Lieut. Glegg of the 16th Native Infantry. The document is dated Allahabad, Dec. 16, 1822.

"On Monday a strong female goat was procured, and deprived of access to food from that evening till the morning of the following Thursday. Wednesday, I accompanied Dr. Tytler into the city, where we purchased two rupees'-worth of Bengal rice out of one of the common shops. There were two species of this grain; one a reddish coarse rice, selling at the rate of sixteen seers for the rupee; the other a finer kind, whiter, and to appearance pretty good rice, selling at eleven seers for the rupee. A mixture of the two kinds of rice, in equal parts, was placed before the animal on Thursday morning. During the day it continued to eat this grain, and occasionally drank some water. The following day its appetite greatly diminished; the two following days it ate nothing, and to me it seemed affected with a disease similar to the distemper in cattle. During the night between Sunday and Monday it uttered most distressing cries at intervals, as if in great agony; and in the morning it was found dead. Within twenty-four hours after eating the rice the animal's bowels were strongly affected, as the evacuations were changed to what in this country is called bilious; which appearance continued and increased till the death of the animal. On opening the bowels, the contents of the stomach and bowels consisted merely of rice, mixed with a little dirty fluid, in an undigested state. The quantity of rice eaten by the animal did not exceed two seers.

"This experiment has greatly tended to convince me of the accuracy of Dr. Tytler's opinion."

"*Attestation of two Experiments by Dr. A. C. Duncan, Hon. Company's Service.*

"You have most certainly proved that rice of this description is capable of producing morbific effects on the animal economy, and apparently in proportion to the quantity taken. Many objections might be urged against the experiments; but they cannot invalidate the fact that the rice produced a diseased action in the goat, and the result of your experiments is well calculated to draw attention to the effects produced on the human system by grain used as an article of diet. The effect produced in the first experiment was undoubtedly dysentery, from irritation of the stomach and bowels; and the preservation of the life of the animal may be in a great measure ascribed to the evacuation of the noxious matter. The second experiment, in which the animal died, was very interesting, as it bore so great an analogy to those cases of cholera, where the patient is cut off suddenly, and without previous evacuation. The most remarkable circumstance, however, is the striking resemblance of the matter contained in the stomach of the animal to that evacuated by cholera patients, by vomiting and purging. The similarity was so remarkable, that I have no hesitation in saying that had any practitioner witnessed such a discharge in a patient, he would have pronounced the case to be cholera.—Gorrukpore, May 24, 1829."

Section the *fifth* contains "facts demonstrating that pestilential vapours, directly destructive of animal life, are, under certain circumstances, emitted by rice." In the holds of ships their effect is said to be highly detrimental. We have already given an instance of this in the statement of Captain Bowie.

Sections the *sixth* and *seventh* consist of extracts from authors, with remarks, relative to the diseased state of certain farinaceous grain in Europe, and its deleterious effects—a deeply-interesting object of medical research, and one which will ere long obtain the anxious and careful attention of Parliament. Section *eight* contains a conjecture that the deleterious properties of "rice ergoté," arise from a noxious principle inherent in the grain. Section *nine* details, at length, numerous circumstances connected with the appearance of the cholera at Allahabad, the most striking features of which have already been given in our reports of the Doctor's speeches. They are conclusive and unimpeachable. Section the *tenth* treats of disease produced in other countries besides India, by the use of rice, and tending to confirm the existence of a *morbus oryzeus*. The crews of vessels stationed or resting at *St. Helena* in Sept. 1818, for instance, were compelled to use a diet two thirds of which consisted of rice. Severe dysentery was the result, and, in *Arabia*, in 1821 the "epidemic cholera did incredible mischief,"—the food of the Arabs being either rice from Bengal, or the large red sort grown on the Malabar coast."

Section *eleven* details many facts respecting the poisonous tunic of rice known by the name of *kun* and *koora*. This tunic, which is so common that probably not a bag of rice is exported from Bengal which does not contain a portion of it, either in its natural form, or as a dust mixed with rice to destroy the insects which pervade masses of that grain, contains not a particle of farinaceous matter, and is impregnated with an acrid essential oil, of a highly poisonous quality, "allied in its properties," Dr. Tytler thinks, "to the oil of tobacco, or bitter almonds." Section the *twelfth* briefly details the author's views of the treatment of the "morbus oryzeus or "epidemic cholera morbus:—which, according to the experience of Dr. Tytler in various regions of Hindoostan, should consist "in expelling, with the utmost promptitude, the noxious ingesta from the stomach and bowels of the patient, and supporting the sufferer's strength, at the same time, with some warm cordial and stimulating drink."

Such is a summary of the contents of this interesting pamphlet. It is, in truth, a book of facts on the subjects of which it treats. It was the object of the author to direct the attention of English medical practitioners to one of the causes of the devastating disease in India designated "cholera morbus." It has been his object also, to direct the minds of medical inquirers to the maladies which are produced by the consumption of unwholesome food. In both instances Dr. Tytler has been perfectly successful. In short, his discoveries have opened a new field for investigation, and it is impossible that it can long remain in its present unscrutinised state. An exploration of this unknown land is the more necessary, as we have in the pamphlet before us most clearly explained the *cause* of a disease which has been deemed to be identical with the pestilential malady which has spread terror among the whole human race,—a cause not even suspected to exist in *any* article of food until those facts were exhibited, an account of which is now presented by Dr. Tytler to the medical profession. Where or when the inquiry which is now in progress will terminate it is impossible to ascertain, but this is certain, that it will henceforth be conducted to useful, and, frequently, to remarkable, results. Is Asia the only quarter of the globe in which diseased grain is the product of unhealthy seasons? We believe not, and, after perusal of this pamphlet, which the Doctor, in a spirit of philanthropy rarely equalled, has published at so low a cost to the purchaser as one shilling,—not sufficient, we expect, to cover the expenses of printing, paper, and advertisements,—there are few reflecting persons, who would not experience confidence and satisfaction if they commanded the opportunity of inspecting the grain from which their *bread* is manufactured.

In soaring through the air, and into the clouds, for the causes of certain diseases, which rage with relentless fury in particular towns and districts, it is now evident that we *may* have been pursuing the shadows, while the substance has been within our grasp.

In earnestly recommending this pamphlet to notice, we express our full concurrence in the opinion recently communicated by one of the most eminent of our physicians, that Dr. Tytler, by his extraordinary labours, and the zeal and love of truth which he has displayed in conducting them, must be regarded as one of the benefactors of mankind.*

WESTMINSTER MEDICAL SOCIETY.

Saturday, December 21, 1833.

Mr. Pettigrew in the Chair.

After the Secretary's minutes of the preceding evening's proceedings had been read,

Dr. James Johnson rose, and addressing the chair, said—As it sometimes occurs in the houses of parliament that members are allowed to make remarks on the reports or criticisms which appear in the

* The subject has obtained much attention in Edinburgh, and the following opinion has, amongst others, been communicated to Dr. Tytler by that eminent physician Dr. Sanders:—

"I have read Dr. Tytler's Essay on the Deleterious Effects of Rice, and am satisfied that he has proved, incontrovertibly, that certain species of rice, used as food, are destructive of human life, as well as powerful agents in producing disease, and, farther, that the subject *demands* the serious attention of every government.

"James Sanders.

"Edinburgh, 15, Duke Street, Dec. 11, 1833."

newspapers relative to the debates in those houses, I am induced to hope that a similar privilege will be extended to a member of this Society, who has been attacked in a weekly journal, this day, under the following circumstances. The ground of attack is contained in the observations made by me in this room previous to moving the eighth resolution, in the course of the late debates on medical reform. The periodical to which I allude has seized on two passages printed some years since in the pages of the medical journal which I conduct, and, contrasting them with certain other passages contained in a report of my observations in prefacing the resolution, attempts to show that I have contradicted myself in a flagrant manner, and have thereby proved myself to be highly inconsistent in the expression of my opinions on medical reform, and especially on medical education. I need hardly say, sir, that in such a journal as mine it is quite impossible for the editor to be answerable for every paragraph it may contain. In no such periodical, in fact, can perfect consistency of sentiment be preserved in every page, for the writers necessarily employed on it must be numerous, and cannot be subjected to the unlimited control of one man. For instance, in the *Quarterly* and *Edinburgh Reviews*, the most incongruous opinions and statements might be detected in any one volume, and yet everybody knows that the views of the editors themselves are, on those occasions, diametrically opposite to the authors of the inconsistencies. On the present occasion it happens that I have the opportunity of proving that the charge of inconsistency against me is totally unjust, for the paper in the *Medico-Chirurgical Review*, whence the quotations have been made, was an anonymous paper, and was not written by me, (*hear, hear,*) but by the late Dr. Dill, the assistant or house-physician of the *Fever Hospital*. It will be said, perhaps, "Yes; but this is only assertion; what proof have you to show that the sentiments contained in Dr. Dill's paper were not your own sentiments?" Such is probably the question of my enemies, if enemies I have any; and I have reason to believe that I am not singular in this respect. My answer is, that I am able to prove the truth of my statement; for in the very same volume of the review—the sixth volume, occupying from page 569 to 574—I have written and inserted a paper avowedly in the name of the editor himself, and distinctly stated to be written by me, in which I maintained every iota of what I lately contended for in this Society; contending, particularly, for the institution of one uniform system of education both for physicians and surgeons, (*hear, hear,*) expressing the same "levelling" opinions which I have uttered within these walls. And those sentiments, moreover, were nothing more than I had published as far back even as the year 1817, and again, with additions to them, in 1827, and yet again in 1828.* Nay, often since that time in the *Review*. Yet all these instances has the editor of the weekly journal passed over, without taking the slightest notice of the evidence they afford of my own sentiments, in order that he might make the present attempt to show that I am an inconsistent man. Now, Sir, as I wish to stand well with the Society, as one of its members, I have felt it my duty to notice the present attack,—the first time I have ever replied to the vituperations directed against me in the journal in question. Seventeen years ago I stated my opinions on the condition of medical education, and then on a far broader basis even than I have done now, and yet has this charge of inconsistency been brought against me. But there are reasons, Sir, for these things. A few years ago, a violent hostility existed between THE LANCET and the *Medico-Chirurgical Review*. To me it was a source of regret, and at length I saw that that hostility was disadvantageous to society and to the profession, and I at once ceased from any further war (*hear, hear*), and I think that society at large has greatly benefited by the arrest of all feelings of enmity between the two journals. (*Hear, hear, hear.*) Now in the weekly journal which this day con-

* "We maintain, then, in the first place, that the PRACTICE OF MEDICINE necessarily includes a knowledge of surgery; secondly, that the PRACTICE OF SURGERY necessarily includes the practice as well as the knowledge of medicine; and thirdly, that the PRACTICE of these two great artificial divisions, necessarily includes a knowledge of all the other divisions, however designated as subordinate, auxiliary, or collateral. Midwifery, pharmacy, chemistry, medical botany, ophthalmic, acoustic, and dental surgery, &c., are all as arbitrary distinctions as those of the unlettered butcher, the erudite Bonetus, or the experienced Baillie. To these separate branches let men attach themselves (after being grounded by education in all the others,) as interest, inclination, or individual genius, may prompt. But, for our parts, we shall make no distinction between the different members of the same family." — *Med.-Chir. Review, Vol. VIII. Jan.* 1828.

tains the article against me, not one word was ever uttered to my disparagement until hostilities between the *Medico-Chirurgical Review* and THE LANCET ceased, but from that moment the hebdomadal in question has lost no opportunity of censuring and even abusing me. Yet, Sir, the editor of that very journal, * my present accuser, on the occasion of a late vacancy in the office of physician to *St. George's Hospital*, called on me to solicit my vote for the appointment, which vote I gave him without hesitation. I beg to apologise for troubling the Society with these statements. (*Hear, hear.*) Notwithstanding the conduct of that periodical, I have never once retaliated—never have until now noticed or replied to its attacks. (*Applause.*)

The CHAIRMAN.—I am sure the Society feels regret at any unfair remarks which may be made against a member who communicates so much information to it, and with so much candour at all times, as Dr. Johnson, who need not, I am satisfied, vindicate a character which stands so high, against the attacks of the journal referred to. (*Hear, hear.*) I have not seen the journal, and do not know of the remarks, and I can only say that it is due to Dr. Johnson that the statement he has now given should be made public in as full a form as possible; and, at the same time, I trust that our Secretary will let the circumstances be rightly recorded on the journals of the Society. (*Hear, hear.*)

Want of space compels us to reduce the further notice of the proceedings of this evening to the following brief statement:—Dr. Epps gave the subjoined notice of a subject which he wished to bring forward at the meeting of the Society on Saturday, the 4th of January next:—

"That the sense of the Society be taken upon the question, Whether it be consistent with the regulations of this Society, that a ballot be taken upon a resolution upon an evening succeeding to the declared passing of the said resolution; more particularly, as another resolution was discussed and passed on the same evening, *after* the resolution referred to was declared passed by the Chairman."

This notice was put by the Chairman as a motion to the meeting, when, after opposition from Dr. Gregory, it was "carried," without the show of a single hand against it. The Society was then adjourned to Jan. 4.

The following statement has been published by Dr. JOHNSON. It is worthy of attention. If such be the leader what must be the followers?—

"Dr. Gregory undertook, in Committee, to draw up a resolution, from two or three propositions, that might meet the wishes and concentrate the ideas of all present. This resolution was the 8th, or ONE-FACULTY resolution, which Dr. Johnson in Committee, undertook to move; and it was this same resolution which was afterwards opposed by Dr. Gregory in the general meeting. The magnates of monopoly are now openly boasting everywhere, that Dr. Gregory has outwitted and knocked up the reformers—by first apparently coinciding with—nay, actually *penning*,—the resolution which he afterwards caused to be thrown overboard in the Society."

LIGATURE OF THE COMMON ILIAC ARTERY.—The lady, whose common iliac artery Mr. Guthrie tied on the right side, on the 24th of August last, for a large gluteal aneurysm, (a report of which appeared in THE LANCET of Aug. 31, page 739,) left town on Saturday, the 21st inst., for Scotland, in good health and spirits. The tumour has diminished three-fourths in size—the pain she experienced in the foot and leg, from the pressure of the aneurysm on the great sciatic nerve, has nearly subsided—the pulsation of the femoral artery, immediately below Poupart's ligament, is as distinct and full as that on the opposite side.—*Med. Chir. Review*, Jan. 1834.

BIOGRAPHY.—Died, at Madras, on the 24th of August, 1833, James Hennen, Esq., assistant-surgeon of His Majesty's 57th regiment, youngest son of the late Dr. Hennen, inspector of military hospitals. He was ardently devoted to the study of his profession, but was suddenly snatched away in the midst of his pursuits, deeply regretted by all who had the pleasure of knowing him. Though he had been but a short time with his regiment, yet his zeal and professional attainments, his amiable and gentlemanly deportment, had endeared him not only to all his brother officers, but to every individual in the regiment, and he has left an irreparable blank in his family.

CORRESPONDENTS.

Dr. Fergusson's letter is unavoidably postponed.

ERRATUM.—In Sir Charles Bell's Lecture, page 480, col. 2, line 12, *for* hardly any voice, *read* hardly any variety in the tone of the voice.

* Roderick Macleod.—*Rep.*